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CLINICS.

Clinical Lectures.

THE RELATION OF CLEANLINESS TO THE PREVENTION OF PUERPERAL SEPTICÆMIA.

A CLINICAL LECTURE.

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ONE of the greatest steps in the progress of modern obstetrics is the full recognition, which has at last been brought about, of the causal relations of septic absorption to the great train of inflammatory and febrile troubles, attendant upon the puerperal condition, and upon the after stage of abortion, embracing those fearfully fatal maladies which have, since obstetrics could be considered to be an art, been the opprobrium of that department of medicine. When we bear in mind that the process of parturition is simply a physiological one; that it is to a human being what the ripening and falling off of the fruit is to the tree; that it is really but a natural function of the reproductive organs, and ought not to be attended with any more danger than the functional operation of any other organs, provided the mother be placed in circumstances favourable for the free course of nature's laws;—we find it a lamentable thing, surely, and a source of the deepest humiliation, that there can arise in the newly delivered woman, a disease, whose source we cannot trace, whose outburst we cannot anticipate, whose course we cannot control; further and more deplorable, that the disease may attack the healthiest, the most robust, the most buoyant, the woman of best previous condition for health, the woman surrounded by the most favourable relations for resisting pestilence, the woman with everything to live for and everything to make her live; that it converts the simple physiological process, which nature ought to guarantee and protect against danger, into a cause of agonizing death, and into the house where the highest joys of life have just been realized, brings, as it were in an instant, the deepest grief and desolation.

We cannot wonder, then, that for ages the teachers and practitioners of the obstetric art should have felt this reproach and bestirred themselves earnestly to remove it. And, we find upon this one subject continuous discussion and the arising from time to time of opposing schools as to the causations and the treatment of this fearful scourge, each school making

an honest and conscientious effort to get at the true cause from its own stand-point of observation, and each founding its treatment upon reasonable deductions from its views of the pathological conditions. We have had in turn the earnest partisans of the doctrine of acute inflammation, of specific fever, of contagion, of epidemic influence, of typhus, and of thrombosis; and the appropriate treatment insisted upon for each. But the true causation of the puerperal diseases was first suggested by Semmelweiss, as being the absorption from open surfaces of decayed animal matter, resulting in a true blood-poisoning, operating either by the production of an erysipelatous inflammation at the point of absorption and gradually travelling to adjacent tissues, and secondarily through the lymphatics and by blood-infection producing a blood-phlogosis and death; or, when the poison was exceedingly virulent being carried by absorption directly into the blood without the previous, or even intercurrent, development of any local phlegmasia, going on to rapid blood-disorganization and death. In other words, he announced the theory that all puerperal fevers, peritonitis, arthritis, cellulitis, and the whole range of those serious lesions appearing a few days after labour, attended by high temperature, increased pulse rate, and various local inflammations, are merely different forms and manifestations of one and the same pathological condition, and that that was septicæmia. The profession, especially the English and the American teachers, were slow in adopting this view, but it has now become so thoroughly proven by clinical observation, and comparative symptomatology, that but little opposition is made to its recognition. It is agreed then at present that these diseases are precisely analogous in their causation, their pathology, their course of development, their urgent need of prophylaxis, and their treatment, to septic inflammation and fever found in surgical practice, their differences dependent only upon the peculiarity of the tissues involved in parturition, upon their exceeding capacity for absorption and transfer of poison, and upon the many points likely to be presented where such absorption may take place.

Another consideration in connection with these diseases, is the vastly greater number of cases needing care than in any other class of practice; also the greater number of sources from whence the poison may be accidentally conveyed to the absorbing surfaces; also, lastly, the greater caution to be used in watching against accidental or thoughtless conveyance from such sources. It is upon this last point which I wish mainly to dwell in this lecture, that is, upon what is called the antiseptic management of labour and the puerperium, the prophylaxis of septicæmia, which, as in all other antiseptic treatment, means simply the maintenance of cleanliness, as far as possible absolutely, in opened surfaces, until they have either united or granulated.

How to keep the genital canal clean during and after parturition is then the great question in the prevention of this horrible malady. The study of this question involves the consideration of the points where absorption may take place, the sources from whence poison may be conveyed, and the precautions to be used against such conveyance.

First, then, as to the points liable to absorb poison. We know that any surface upon which there is loss of integrity of the external investment (whether epidermis upon the integument, or epithelium upon the mucous membrane) is capable of absorption; and we know well how a very slight opening into the integument upon the hands, a scratch, of which one was not even conscious before, will smart when introduced into any

acrid liquid, or may absorb the fatal poison from the cadaver when it was not even suspected. How much more then might we expect, upon theoretical grounds, that the highly vascular tissues of the genital canal, as we find them in their increased vitality in pregnancy and even more especially during parturition, would be quick to take up any noxious material which comes in contact with fissures or abrasions upon their surfaces; and unfortunately our practical experience gives sad confirmation of the theory. We know how, even in the unimpregnated uterus, the gentlest use of a mechanical dilator may lacerate the mucous surface of the cervix, producing, perhaps, tears that the naked eye could scarcely detect; and we know so well, if any broken-down or decaying tissue from a malignant growth or from the scraping of vegetations, so often recklessly done, should come into contact with this point of opening, how a fire is lighted up, and we are startled by the outbreak of a series of symptoms, the meaning of which we know so well: the high temperature, and the rapid pulse, and the quickened respiration, and then the pain and the tenderness—pelvic and abdominal—and the chill, and the other evidences of the septic metritis, and cellulitis and parametric phlegmon, and, perhaps, peritonitis and death. And yet in the process of labour, we have not only the little fissure to absorb, but also, especially in the primipara, large open surfaces, superadded often one upon another, from the cervical canal to the integument of the perineum. Now these are altogether unavoidable to a large extent; in a greater or less degree we may consider that they are present in every case of labour. There is probably not one case in a thousand in which there is not before the process is completed some solution of continuity. This may occur very superficially without any bleeding, but when there is blood there must necessarily be a surface to bleed, and how very rare is it to go through the management of a case where there is no stain of blood before the head is expelled. The "show," that is, the plug of tinged mucus, which sometimes and often amounts to a free discharge of blood, is recognized by the monthly nurse as the diagnostic sign that labour has begun, so uniformly is this present.

This, then, is the beginning of the lacerations, and the first appearance of blood in normal labour, or when there is no premature placental detachment, or mis-implantation of that organ. This discharge arises, undoubtedly, from the rupture of some minute fibres of the uterine lower segment, probably in the expansion of the internal os, as the contractions of this body drive the membranous pouch or the presenting part of the child into it, at the same time loosening up the plug of cervical mucus, which comes away stained with the momentary discharge from the lacerated surface. How far this continues to tear, we cannot say, but I have no doubt that fissures in the canal are much more frequent than we have any positive means of proving, for we often have free, and even profuse, bleedings from the cervix after the expulsion of the placenta, when no fissure in the rim of the os can be detected, yet the firm contraction of the uterine body and the bright red colour of the flow, and sometimes even the arterial jet, show that it cannot come from patulous venous sinuses opening upon the placental site. There is no blood from these tears while the head is passing, because the compression of the surface then prevents it. As the distension goes on, just before the os is fully open, a violent expulsive effort will often drive the presenting part of the child so forcibly, that the most prominent projection upon the circumference of the head-globe, making a sudden strain upon the rim, where it impinges, will tear

through, either in the line of a fissure already made, or, starting an entirely new laceration, until a gap is produced involving the whole distance from the rim to the vaginal reflection. And then, after the head has cleared the os uteri, and meets the resistance of an undilatable canal, and especially in the posterior occipital position, when the forced flexion drives the occiput at each recurring pain violently against the posterior vaginal wall, how often does the finger detect the opening of a little fissure upon the mucous surface, as the head recedes after the subsidence of the pain; and with each successive pain finds the fissure deepening into a furrow, which causes the observer great anxiety for the safety of the perineal body, lest it should involve the deep tissue of the recto-vaginal septum, and unless prevented by judicious labial incisions, even cut through the perineum and the sphincter into the rectum. Then as the head passes the vulvar outlet, there may be minor separations of tissues, even if the perineum proper be saved; and sometimes when no laceration can be found upon the mucous surface, the perineal integument in the line of the raphé may yield superficially before the great tension.

Thus we see how many ways there are in which the surfaces of the parturient canal may be opened for absorption of poison, and we have in addition to all these the large placental site, covering one-fourth of the whole superficies of the uterine cavity. It will be easily recognized, why in the primipara, with her rigid tissues never before subjected to dilatation and strain, the probabilities will be vastly greater of vaginal and vulvar, and somewhat greater of cervical, laceration, than in the pluripara, with her previous distension.

Having now considered briefly the causes and the character of the various absorbing surfaces in the lying-in woman, let us call to mind the sources from whence poison may come. The septic diseases of childbed have been divided into *autogenetic* and *heterogenetic*, more for the ease of mind of the accoucheur than from any real distinction in the nature of the poison or of the results that attend its absorption. The autogenetic infections are those which originate from the absorption of poison generated in the body of the mother; the heterogenetic, those in which the septic matter is conveyed to the maternal tissues from extraneous sources through the neglect or ignorance of the accoucheur or the nurse.

From her own fluids the mother may be infected during her labour, when she has been the subject of any disease of her uterine or vaginal canal, attended by a discharge or casting-off of broken-down or sloughing tissue; a scirrhus or epithelioma which has undergone softening within the cervix, upon its external surface, or anywhere in the vaginal canal, or upon the vulva, where the discharge may come in contact with any open surface or point; a polypus or other non-malignant growth, from whose surface there may be a discharge, or which may be caused to slough from pressure in labour; and, more commonly, any ulcerated surface or erosion upon the cervix or vagina, discharging pus or a muco-purulent discharge, which, if allowed to remain in the vagina for any length of time, becomes virulently poisonous.

There is another autogenetic source of infection during the process of parturition, to which, as far as I know, attention has never been called, and yet, which I doubt not, is one of the most fertile causes of septic fever in cases of prolonged labour. This is the accumulation and continued exposure to atmospheric influence in contact with the warm tissues of the mother, of the *natural and healthy discharge* of mucus with its admix-

ture of blood from the cervix and vagina, while the tissues are gradually relaxing for the passage of the child. This discharge, perfectly healthy and clean, as it exudes from the vaginal canal, bathes the vulva and perineum, and the thighs and pubic region; it agglutinates the tissues, matting up in thick adhesive masses upon the labial surfaces, as can be seen when the patient is cleansed after her labour, if it has not been scrupulously cleansed before, requiring vigorous efforts with soap and water to remove it. It remains exposed to the warm air of the chamber upon the warm surface of the body. Decay, with its resulting sepsis, must soon, under these circumstances, begin its work, and the poisoned moisture outside will readily impart its virulence to the fluids of the lower vagina, and as the head passes through the vaginal canal making its attacks upon the natural barriers provided against infection, and opening one or many doors for the enemy to enter, it is there in full force prepared to insinuate itself into tissues well prepared by their unusual vitality and turgescence to receive it.

Of the autogenetic causes acting after parturition is over, the main source of infection will be the decomposition of the lochial discharge or of clots of blood or pieces of membrane retained in the uterus from deficient cleaning after labour, or from defective involution. This source will, probably, rarely be a fruitful one upon the cervical or vaginal lacerations, for these will begin to suppurate and granulate in their effort at repair in from twenty-four to thirty-six hours (unless made to unite by sutures), and up to that time the lochial discharge rarely becomes foul or likely to infect. After granulation begins there is no absorption, so that it could only be by a re-opening through some disturbance of the granulating surface, such as might occur from the passage of a large clot through the now contracted vagina, or even the presence of large, hard, fecal masses in the lower rectum. But we may easily have, as we can often recognize, septic fever developed by the absorption of poisoned lochia, or of decomposing clots pent up within the uterine cavity, through the imperfectly closed orifices of vessels opening upon the placental site of the too slowly involuted womb. The poisoning from the amniotic fluid from around a dead fœtus is so questionable as to its occurrence that it is scarcely worth while to mention it.

The *heterogenetic* sources of infection will be found in poisonous matter conveyed by the hand or the clothing upon the arm (either an enveloping towel or sleeve) either of the doctor or nurse, or upon the surface of the instruments used either during or after labour. Upon the hands of the accoucheur may be carried any septic matter with which they have come in contact, even for several days, unless care be taken with such methods of cleansing as will be suggested hereafter. The worst and most rapidly violent poisons are those from the autopsies or manipulations of tissues of persons dead with blood-diseases; but any autopsies, or handling of pathological specimens or decaying or broken-down tissues from the body living or dead ought to be avoided as death-carriers by the accoucheur, and, unless under some peculiar circumstances which might rarely render such handling necessary, ought to be conscientiously avoided. But there are equally noxious matters with which the hand of the obstetrician may be unavoidably polluted in the necessary exercise of professional duty, after contact with which the only thing is thoroughly to cleanse. Vaginal examination in cases of malignant disease, especially in operative work, when the hands are long in the vagina; examinations also of any tumours in a

sloughing condition; of a uterus dilated by tents; of any diseased conditions, even of an inflammatory character, with discharge; of simple leucorrhœa; any of these might be the source of such contagion. And especially would I caution earnestly against careless use of the hands after contact with the disorganized clots in a case of hemorrhagic abortion, the foul odour from which is as penetrating and as persistent upon the hands, and the poison as virulent, in my opinion, as that of a sloughing cancer. All discharges from wounds of the body in surgical practice should also be sedulously removed. In short, organic matter of any sort should be removed promptly and thoroughly from the hands of the obstetric practitioner, lest he should be caught with a case unexpectedly, when active detergents and disinfectants could not be obtained as quickly as an examination might be needed.

But infection may take place from the finger of the obstetrician, after the most scrupulous cleansing from any previous infection, by carelessly carrying back to the patient's person her own discharge, in the frequent examinations of a long labour. This is a point upon which I do not think any one has made suggestions, but it is easily seen that if the woman may poison herself by the retention upon her external tissues of decomposing discharges, ever so healthy at first, so may the doctor by carelessly retaining upon the hand these same fluids perfectly harmless previously, return them after they have decomposed under the nails and upon the fingers to the abraded vagina, to poison the woman whom he has been selected to protect. How often have I seen a doctor after an examination simply wipe his hands upon a towel, and make a subsequent examination without any washing in the interval.

So, also, the doctor cannot be too cautious about any sores upon the hands, or upon any part of his person with which the hands may come in contact; granular ophthalmia, otorrhœa, and what may be so easily overlooked, the discharge from a purulent coryza, or more especially from a foul ozæna. There is an historical case, to which I had better call attention in this lecture, of a physician in the early middle part of this century, who, practising in this city, was followed about from case to case almost in scores by the fearful scourge of puerperal fever, as it was then called, a man truly a "colporteur of pestilence," as Professor Schroeder would aptly have termed him; who used every means that could be thought of to cleanse his person, and to purify and regularly change his clothing, but to no effect; his touch was poison, and yet no one ever dared to suggest to him, and perhaps no one ever thought of it until too late, that the reason why his work everywhere was accompanied by the fearful disease, when other men were free from it, might be that he had a foul chronic ozæna, the thoughtless contact of his finger with which could carry poison to the tissues of his patient.

From the clothing both of the physician and nurse pestilence can be conveyed, and I would suggest that in all cases the sleeve of the doctor should be drawn up a short distance at least from the wrist and the latter closely enveloped in a napkin tightly held between the fingers and the palmar surface of the hand, and not pinned permanently. For upon the sleeve may accumulate little by little discharges from various examinations and operations, unconsciously to the physician, which when brought to lie against the mother's tissues, warm and moist, would be a fruitful source of infection. If the arm is covered with a napkin, to avoid a disagreeable impression, the sleeve may be rolled up to the elbow. But my suggestion

about the napkin not being kept permanently upon the arm during many successive examinations follows naturally upon what I had said before about the neglect to wash the hands during the intervals between the same. A towel saturated around the wrist with vaginal discharge would itself be a fertile source of infection if again brought into close proximity to the genitalia at a subsequent touch. Where at all possible, a towel even used in a lying-in chamber by the physician should be carefully put out of reach until the labour is over, or if it must be used again, thoroughly washed and disinfected in the interval. How often this is neglected by a nurse, or others in the room, a towel already soiled ever so little with blood and discharges, being handed to the accoucheur again to wipe his hands before making an examination.

Monthly nurses cannot be too scrupulous in regard to personal cleanliness in going from one case to another, and especially in going directly from the dressing of suppurating wounds or cases attended with foul discharges. In attendance upon such cases they should always wear wash dresses, and put them through the boiling process before again wearing them.

In enumerating the sources of puerperal septicæmia, I do not mean to ignore certain meteorological or atmospheric influences, present during *epidemics* of puerperal fever, so called, which predispose the patient to respond more readily to the poisonous impression of septic absorption, nor do I question the predisposing influence of malaria, and some of the eruptive fevers, to magnify such a septicæmia as in a majority of cases used to be developed upon the third day, and erroneously called *milk fever*, but which nature could often throw off unaided,—to magnify, I say, such a condition into a malignant case of septic fever, perhaps ending fatally. But I am thoroughly convinced that whatever be the predisposing condition, the immediate exciting cause of the fearful malady in all its various phases, is the absorption by open surfaces of disorganized animal matter.

It is easy to see why primiparæ are more likely to be victims to this disease than pluriparæ, when we consider the greater likelihood of injury to the mucous surface of the genital canal, the longer period of exposure of the vulvar orifice to atmospheric influence before the external surface is cleansed, and the necessity for more repeated and prolonged manipulation especially upon the perineum, in the neighbourhood of which the tissues are so very apt to be more or less impaired in their integrity.

We arrive then finally at the purpose of this lecture, to see how we can most effectually carry out that indication which must be the clear one to any mind, the PREVENTION of such poisoning; and the essential means of such prevention is *cleanliness*, the keeping clean of the tissues before and after the production of lesions of their surface, and of everything that comes in contact with them; the purifying and removal of all morbific materials, and the prevention by suitable means of their reproduction. The antiseptic treatment in surgical cases is really only a systematic method of cleanlinesss.

First, in regard to the care of the physician's person. After laying down the rule that the hands should be carefully washed and cleansed with disinfectants after handling any animal matter, I would still further say that the person of the patient should not be approached without a most thorough and complete repetition of such process, between which cleansing and the examination of the patient, nothing should be touched except what is known to be perfectly clean. No driving should be done, no

gloves worn, nothing should come in contact with the hands of a doubtful character. This cleansing should be done by soaking the hands and arms first in warm water, then with a *stiff* nail-brush with soap, scrubbing most carefully under the nails, where the poison is retained most persistently; and then the whole hands and wrists; a clean water is then to be used with the nail-brush, and the hands allowed to soak in it, medicated in the proportion of two and one-half per cent. of carbolic acid, or six to eight per cent. of good recently-made Labarraque's solution; that is about seven fluidrachms of a ninety-five per cent. solution of crystals of the former, and about a fluidounce of the latter to the pint of water. I always order as part of the preparation for labour, a bottle of each of these antiseptics to use under different conditions as may be required; and usually prepare for myself in each case, some washed lard carbolized to about five to ten per cent. The hands should be washed as directed always before the first examination, and after each examination and again before the subsequent they should be thoroughly washed with the nail-brush in one solution or the other. My preference is rather for the chlorinated water, as it is a more thorough disinfectant, and in such proportion as is necessary for the purpose, does not burn the skin nor obtund the tactile sense, as does a carbolized solution. Any instruments which can possibly be required in the labour, forceps, vectis, or catheter, should be treated in the same way with brush, soap, and disinfectant solution always both before and after use. Neither hands nor instruments should ever be wiped upon a towel previously used or soiled; it is better to let both go unwiped, where not a matter of importance, than to risk wiping them upon a doubtful towel.

During the labour systematic cleanliness with the patient should be rigidly observed. If practicable at the onset of parturition, she should take a general bath at about 95 to 100° F., which is useful not only for detergent purposes but also as a promoter of the action of the skin, and as relaxing the muscular fibre, and the tissues generally. After a rectal enema a vaginal enema either of the carbolized or chlorinated solutions above suggested may be given, if the membranes are not ruptured; if they are, or if there be any doubt about it, we should not risk the throwing in of these solutions into the uterine cavity in contact with the fœtus. But the external genitalia should be frequently bathed with one of these solutions, and a soft cloth moistened with the same passed up inside the vaginal cavity from time to time, as far as the finger of the physician or nurse can carry it. The use of the *spray* during the whole process of labour, seems to me to be not only unnecessary, but very annoying to the patient, if done thoroughly enough to be of any service. It involves the necessity of keeping it under the constant observation of an assistant and the genitalia constantly exposed to view; the bed clothing and the patient's person kept all the time wet with it. If a carbolized spray be used the patient's tissues would be irritated and benumbed, and to keep a patient in the agonies of labour in a fixed position in order for this, which is an annoyance to her at any rate, would be a tax upon her forbearance which would be unreasonable. Then again, if it be only during the manipulation of the accoucheur, a carbolized spray would be just as annoying to him as it would be to the patient; saturating the clothing and benumbing the hand and arm. Instead I find perfect satisfaction in keeping at the side of the bed, during the whole time of my manipulations, a dilute Labarraque solution, double the strength of that mentioned; I put into a half-pint toilet cup an ounce of the solution, and fill the cup nearly full of water; in this I keep a small

soft cloth of old muslin or linen; whenever I make the touch I dip the finger into the cup, and from time to time bathe the genitalia with the cloth wrung out in the liquid removing all mucus and blood accumulated, and as the head is descending through the vaginal canal I frequently pass the cloth freshly wrung out in the cup up into the cavity as far as to reach the head. As the head is passing the vulva, distending the perineum, I often keep the cloth in continuous contact with the tissues. Thus there is no possibility of infection taking place either autogenetically or heterogenetically. If any decomposing mass were in the pelvic cavity, a scirrhus or epithelioma, the cleansing would have to be even more vigilant and repeated.

After the delivery of the placenta, I wash out the uterus at once with a syringe with hot water at 115° to 120°, either chlorinated, or acidulated with vinegar, about twenty-five per cent. I formerly used carbolic solution for this, but in two cases in the last year, I saw symptoms which confirmed Fritsch's statements as to the absorption of carbolic solution by the uterus, in the appearance of carbolic poisoning immediately.

For four days after the labour I have the nurse wash out the vagina with carbolized or chlorinated water about 110° F. every four to six hours, If any offensive lochia appear with the least symptom of septic poisoning, I wash out the uterus with the strong ten to fifteen per cent. Labarraque with a double canulated uterine tube, using a siphon generally in preference to a forcing syringe, to avoid the passage of the fluid into the peritoneal cavity.

ADDRESS ON OUR MEDICAL LITERATURE.

Delivered at the International Medical Congress.

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When I was surprised by the honour of an invitation to address this Congress, my first thought was that it must be declined; for the simple but sufficient reason that I had nothing to say that would be worth occupying the time of such an assemblage as it was evident this would be. But while thinking over the matter, and looking absent-mindedly at a shelf of catalogues and a pile of new books and journals awaiting examination, it occurred to me that perhaps some facts connected with our medical literature, past and present, from the point of view of the reader, librarian, and bibliographer, rather than from that of the writer or practitioner, might be of sufficient interest to you to warrant an attempt to present them; and, the wish being probably father to the thought, I decided to make the trial.

When I say "Our Medical Literature," it is not with reference to that of any particular country or nation, but to that which is the common property of the educated physicians of the world as represented here to-day—the literature which forms the intra- and inter-national bond of the medical profession of all civilized countries; and by virtue of which we, who have come hither from the far West and the farther East, do not now meet for the first time as strangers, but as friends, having common interests, and though of many nations, a common language, and whose thoughts are perhaps better known to each other than to some of our nearest members.

It is usual to estimate that about one-thirtieth part of the whole mass of the world's literature belongs to medicine and its allied sciences. This corresponds very well to the results obtained from an examination of bibliographies and catalogues of the principal medical libraries. It appears from this that our medical literature now forms a little over 120,000 volumes properly so called, and about twice that number of pamphlets, and that this accumulation is now increasing at the rate of about 1500 volumes and 2500 pamphlets yearly.

Let us consider the character of this annual growth somewhat in detail, first giving some figures as to the number of those who are producing it.

There are at the present time scattered over the earth about 180,000 medical men, who, by a liberal construction of the phrase, may be said to be educated—that is, who have some kind of a diploma, and for whose edification this current medical literature is produced. Of this number about 11,600 are producers of, or contributors to, this literature, being divided as follows: United States, 2800; France and her Colonies, 2600; the German Empire and Austro-Hungary, 2300; Great Britain and her Colonies, 2000; Italy, 600; Spain, 300; all others, 1000. These figures should be considered in connection with the number of physicians in each country; but this I can only give approximately, as follows: United States, 65,000; Great Britain and her Colonies, 35,000; Germany and Austro-Hungary, 32,000; France and her Colonies, 26,000; Italy, 10,000; Spain, 5000; all others, 17,000.

It will be seen from these figures that the number of physicians who are writers is proportionately greatest in France and least in the United States. As regards France, this is largely due to the requirement of a printed thesis for graduation, which of itself adds between six and seven hundred annually to the number of writers.

Excluding popular medicine, pathies, pharmacy, and dentistry, all of which were included in the figures for the annual product just given, we find that the contributions to medicine, properly so called, form a little over 1000 volumes and 1600 pamphlets yearly.

For 1879 Rupprecht's Bibliotheca gives as the total number of new medical books, excluding pamphlets, periodicals, and transactions, 419; divided as follows—viz.; France, 187; Germany, 110; England, 43; Italy, 32; United States, 21; all others, 26. These figures are, however, too small, and especially so as regards Great Britain and the United States. The Index Medicus for the same year shows by analysis that the total number of medical books and pamphlets, excluding periodicals and transactions, was 1643; divided as follows: France, 541; Germany, 364; United States, 310; Great Britain, 182; all others, 246. This does not include the inaugural theses, of which 693 were published in France alone.

The special characteristics of the literature of the present day are largely due to journals and transactions, and this is particularly true in medicine. Our periodicals contain the most recent observations, the most original matter, and are the truest representations of the living thought of the day, and of the tastes and wants of the great mass of the medical profession, a large part of whom, in fact, read very little else. They form about one-half of the current medical literature, and in the year 1879 amounted to 655 volumes, of which the United States produced 156, Germany, 129, France, 122, Great Britain, 54, Italy, 65, and Spain, 24. This is exclusive of journals of pharmacy, dentistry, etc., and of journals devoted to medical sects and isms. In a table I have drawn out it appears that the total number of volumes of medical journals and transactions of all kinds was, for the year 1879, 850, and for the year 1880, 864. The figures for 1880 are too small, but the real increase is slight. During the year 1879, the

total number of original articles in medical journals and transactions which were though worth noting for the Index Medicus was a little over 20,000. Of these there appeared in American periodicals 4781, in French 4608, in German 4027, in English 3592, in Italian 1210, in Spanish 703, in all others 1248. The figures for 1880 are about the same. It will be seen that at present more of this class of literature appears in the English language than in any other, and that the number of journal contributions is greatest in the United States. The actual bulk of periodical literature is, however, greatest in Germany, owing to the greater average length of the articles. With regard to the mode of publication, I will only say that in all countries except Spain the greater number of medical periodicals are monthly, while in Spain they are semi-monthly. It is this periodical literature which, more than anything else, makes medicine cosmopolitan, and although as regards new discoveries or methods of treatment it is still somewhat farther from London or Berlin or Paris to New York than it is from New York to either of these places, the discrepancy is gradually becoming less.

Many of the medical journals are very short lived, but the total number is increasing. In 1879, 23 such journals ceased, but 60 new ones appeared, and in 1880 there were 24 deaths and 78 births in this department of literature. Over one-third of this fluctuation occurs in the United States alone, France being next in the scale, Spain third, and Italy fourth, while Great Britain is the most stable of all.

This merely quantitative classification gives of course no idea as to the character, and very little as to the value of the product. Let us now consider it by subjects. During 1879 there were published 167 books and pamphlets and 1543 articles relating to anatomy, physiology, and pathology—that is, to the biological or scientific side of medicine. Dividing this again by nations, we find that Germany produced a majority of the whole, France being second. The proportionate production by nations of this class of literature is perhaps better shown by an analysis of the bibliography of physiological literature for the year 1879, as published by the *Journal of Physiology*. This shows 59 treatises and 500 articles in German, 17 treatises and 227 articles in French, 5 treatises and 77 articles from Great Britain, 8 treatises and 41 articles from Italy, and 2 treatises and 24 articles from the United States. The number of authors for this product was—German, 393; French, 119; English, 59; Italian, 39; United States, 19; all others, 41. For the year 1880 the same journal reports 62 treatises and 452 articles from Germany, 23 treatises and 216 articles from France, 12 treatises and 76 articles from Great Britain, 4 treatises and 51 articles from Italy, 6 treatises and 25 articles from the United States, and 10 treatises and 31 articles from all other countries.[1]

When we turn to the literature of the art, or practical side of the profession, the figures are decidedly different. We find over 1200 treatises and 18,000 journal articles which come under this head, and the order of precedence of countries as to quantity is France, United States, Germany, Great Britain, Italy, and Spain. The tables I have constructed give still further subdivisions, showing by nations the number of works and journal articles upon the practice of medicine, surgery, obstetrics, hygiene, etc., for the years 1879 and 1880, and some of the figures will be found interesting. A marked increase has occurred in the literature of hygiene during the last two years, and this especially in England, France,

[1] The difference between these figures and those of the Index Medicus is due, on the one hand, to the fact that the Journal of Physiology includes articles which are placed under other headings in the Index Medicus, and, on the other hand, to the fact that the Journal has a different standard of excellence from that of the Index, rejecting many articles which the latter must accept as original.

Germany, and the United States. The literature of diseases of the nervous system, of ophthalmology, otology, dermatology, and gynæcology is also increasing more rapidly than that of the more general branches.

It would, of course, be extremely unscientific to use these figures as if they represented positively ascertained and compared facts, the accuracy of which, as well as of the classification, could be verified. They represent merely the opinions of an individual—first, as to whether each treatise or pamphlet included in these statistics was worth noting, and second, as to how it should be classed. Had everything been indexed, the figures, for journal articles at least, might have been nearly doubled; while if the selection had been made by a more severe critic they might have been reduced one-half.

If I had to do the work again I should not obtain the same results. The prevailing error is that as regards journal articles the figures are too large, for some of those included are of so little value or interest that they are, I fear, never read by more than two persons.

Be that as it may, I think we can take them as indicating certain differences in the direction of work of the medical authors of the great civilized nations of the earth; but they must be considered as approximations only; and the statistical axiom must be remembered that the results obtained from a large number of facts are applicable to an aggregate of similar facts, but not to single cases. There will be a certain number of medical books and papers printed next year just as there will be a certain number of children born—and as we can within certain limits predict the number of these births and the proportion of the sexes, or even of monsters—so we can within certain limits predict the amount and character of the literature that is yet to come, the ideas that are yet unborn. The differences are due to race, political organization, and density of population. As Dr. Chadwick has pointed out in speaking of the statistics of obstetric literature, one of the chief causes of the multiplication of medical societies is geographical. "In England it is possible for those who are specially interested in gynæcology and obstetrics to attend the meetings of the Obstetrical Society of London, whereas in America the distances are so great that this is impossible." Speaking broadly we may say that at present Germany leads in scientific medicine both in quantity and quality of product, and that the rising generation of physicians are learning German physiology. But the seed has gone abroad, and scientific work is receiving more and more appreciation everywhere.

Seven years ago Professor Huxley declared that if a student in his own branch showed power and originality he dared not advise him to adopt a scientific career, for he could not give him the assurance that any amount of proficiency in the biological sciences would be convertible into the most modest bread and cheese. To-day I think he might be bolder, for such a fear would hardly be justifiable—at all events in America, where such a man as is referred to could almost certainly find a place, bearing in mind the professor's remark that it is no impediment to an original investigator to have to devote a moderate portion of his time to giving instruction either in the laboratory or in the lecture room.

Within the last ten years the literature of France, Germany, Great Britain, and the United States has contained much with regard to medical education and the means for its improvement. In all these countries there is more or less dissatisfaction with the existing condition of things, although there is no general agreement as to the remedy. Solomon's question, "Wherefore is there a price in the hands of a fool to get wisdom, seeing he hath no heart to it?" is now easily answered, for even a fool knows that he must have the semblance of wisdom, and a diploma to imply it, if he is to succeed in the practice of medicine, but to insure the value of a diploma as a proof of education is the difficulty.

This evidence of discontent and tendency to change is a good sign. In these matters stillness means sleep or death—and the fact that a stream is continually changing its bed shows that its course lies through fertile alluvium and not through sterile lava or granite.

I have said that as regards scientific medicine we are at present going to school to Germany. This, however, is not the case with regard to therapeutics either external or internal—in regard to which I presume that the physicians of each nation are satisfied as to their own pre-eminence. At all events it is true that for the treatment of the common diseases a physician can obtain his most valuable instruction in his own country, among those whom he is to treat. Just as each individual is in some respects peculiar and unique, so that even the arrangement of the minute ridges and furrows at the end of his forefinger differs from that of all other forefingers, and is sufficient to identify him; and as the members of certain families require special care to guard against hemorrhage, or insanity, or phthisis; so it is with nations and races. The experienced military surgeon knows this well, and in the United States, which is now the great mixing ground, illustrations of race peculiarities are familiar to every practitioner.

Neither the tendency nor the true value of this current medical literature can be properly estimated by attending to it alone. It is a part of the thought of the age—of that wonderful kaleidoscopic pattern which is unrolling before us, and must be judged in connection with it. From several sources of high authority there have come of late years warnings and laments that science is becoming too utilitarian. For example, Prof. Du Bois-Reymond, in his address upon civilization and science, says that that side of science which is connected with the useful arts is steadily becoming more prominent, each generation being more and more bent on material interests. "Amid the unrest which possesses the civilized world, men's minds live as it were from hand to mouth. . . . And if industry receives its impulse from science, it also has a tendency to destroy science. In short, idealism is succumbing in the struggle with realism, and the kingdom of material interests is coming." Having laid down this rather pessimistic platform, he goes on to state that this is especially the case in America, which is the principal home of utilitarianism, and that it has become the custom to characterize as "Americanization" the dreaded permeation of European civilization by realism. If this characterization is correct, it would seem that Europe is pretty thoroughly Americanized as regards attention to material interests and appreciation of practical results. But the truth of the picture seems to me doubtful. Science is becoming popular, even fashionable, and some of its would-be votaries rival the devotees of modern æstheticism in their dislike and fear of the sunlight of comprehensibility and common sense. The languid scientific swell who thinks it bad style to be practical, who takes no interest in anything but pure science, and makes it a point to refrain from any investigations which might lead to useful results lest he might be confounded with mere "practical men," or inventors, exists and has his admirers. We have such in medicine, and their number will increase.

The separation of biological study from practical medicine, which has of late years become quite marked in the literature of the subject, has its advantages and disadvantages. Thus far the former have far outweighed the latter, and both the science and the art of medicine have been promoted thereby. But are not the physiologists, or, as I believe they prefer to be called, the biologists, separating themselves too completely from medicine for the best interests of their own science, in that they are neglecting human pathology? In our hospital wards, and among our patients, nature is continually performing experiments which the most dextrous operator cannot copy in the laboratory; she is, as Pro-

fessor Foster says, "a relentless and untrammelled vivisector, and there is no secret of the living frame which she has not, or will not, at some time or place, lay bare in misery and pain."

Now, while it is true that Professor Foster, in his address before the British Medical Association last year (which address is the clearest exposition of the aims of the physiology of the present day that I have seen), insists upon the fact that all distinctions between physiology and pathology are fictitious, and declares that attempts to divide them are like attempts to divide meteorology into a science of good and a science of bad weather, his conclusion that the pathologist should be trained in methods of physiological investigation seems to me to be only a part of the truth. The tacit assumption is that all, or at least the most important, phenomena of human disease may be reproduced in the physiological laboratory. If this were only true what a tremendous stride would have been taken towards making medicine a science. Unfortunately, it is not so. Many of the most interesting of these phenomena, the most interesting because as yet the most unexplainable, can only be observed in the sick man himself. Nor have the physiologists as yet made much use of that field which ought to be specially inviting to them—namely, comparative pathology, although the literature of the present time already indicates that a change has begun in this respect.

While it is true that to the graduate of thirty years ago much of the physiological literature of the present day is an unknown tongue, it is also true that the physiologist of the present, who confines himself to laboratory work, will find himself distanced by the man who keeps his clinical and pathological studies and his experimental work well abreast.

The increase in both the amount and value of the literature of the several specialties in medicine is readily seen by a comparison of recent catalogues and bibliographies with those of twenty or thirty years ago, and this increase still continues at a greater rate than prevails in the more general branches. There are great differences of opinion as to the relative value of this increase and as to its future effect upon the profession, but there can be no doubt as to the fact. There must be specialties and specialists in medicine, and the results will be both good and evil; but the evils fall largely upon those specialists who have an insufficient general education, who attempt to construct the pyramid of their knowledge with the small end as a foundation. It has been said by Dr. Hodgen that "in medicine a specialist should be a skilled physician, and something more; but that he is often something else—and something less." There is truth in this; truth which the young man would do well to consider with care before he begins to specialize his studies; but, on the other hand, it is also true that the great majority of men must limit their field of work very much and very clearly if they hope to achieve success. The tool must have an edge if it is to cut. It is by the labour of specialists that many of the new channels for thought and research have been opened, and if the flood has sometimes seemed to spread too far, and to lose itself in shallow and sandy places, it has nevertheless tended to fertilize them in the end.

The specialists are not only making the principal advances in science, but they are furnishing both strong incentives and valuable assistance towards the collection and preservation of medical literature and the formation of large public libraries.

Burton declares that a great library cannot be improvised, not even if one had the national debt to do it with—thinks that 20,000 volumes is about the limit of what a miscellaneous collection can bring together, and refers especially to the difficulty in creating large public libraries in America. My experience would show that these statements do not apply to medical books. Of these the folios and quartos of three and four hundred years ago seem to have had great capacity

for resistance to ordinary destructive forces. Perhaps much of this is due to the fact that they are not usually injured by too much handling or perusal. True, they are gradually becoming rarer, but at the same time by means of properly organized libraries they are becoming more accessible to all who wish to really use them, and not merely to collect and hide them away. They drift about like the seaweed, but the survivors are gradually finding secure and permanent resting places in the score of great collections of such literature which the world now possesses. At present the currents of trade are carrying them in relatively large numbers to the United States, where medical collectors and specialists are among the best customers of the antiquarian booksellers of Europe. I could name a dozen American physicians who have given to European agents almost unlimited orders for books relating to their several specialties, and upon their shelves may be found books of the 15th and 16th centuries, which may be properly marked as "rarissime."

Not that the rarest books are by any means the oldest. The collector who seeks to ornament his shelves with the Rose of John of Gaddesden, or the Lily of Bernerd de Gordon, the first folios of Avicenna or Celsus, or almost any of the eight hundred medical incunabula described by Hain, will probably succeed in his quest quite as soon as the one who has set his heart on the first editions of Harvey or Jenner, the American tracts on inoculation for smallpox, or complete sets of many of the journals and transactions of the present century.

Whatever may be the chosen line of the book collector, he is the special helper of the public library, and this whether he intend it or not. In most cases his treasures pass through the auction room, and sooner or later the librarian, who can afford to wait, will secure them from further travel. Thanks to the labours of such collectors, I think it is safe to say—what certainly would not have been true twenty years ago—that if the entire medical literature of the world, with the exception of that which is collected in the United States, were to be now destroyed, nearly all of it that is valuable could be reproduced without difficulty.

What is to be the result of this steadily increasing production of books? What will the libraries and catalogues and bibliographies of a thousand, or even of a hundred years hence be like if we are thus to go on in the ratio of geometric progression, which has governed the press for the last few decades? The mathematical formula which would express this, based on the data of the past century, gives an absurd and impossible conclusion, for it shows that if we go on as we have been going there is coming a time when our libraries will become large cities, and when it will require the services of every one in the world, not engaged in writing, to catalogue and care for the annual product. The truth is, however, that the ratio has changed, and that the rate of increase is becoming smaller. In Western Europe, which is now the great centre of literary production, it does not seem probable that the number of writers or readers will materially increase in the future, and it is in America, Russia, and Southern Asia that the greatest difference will be found between the present amount of annual literary product and that of a century hence.

The analogies between the mental and physical development of an individual, and of a nation or society, have been often set forth and commented on, but there is one point where the analogy fails as regards the products of mental activity—and that is that as yet we have devised no process for getting rid of the exuviæ. Growth and development in the physical world imply the changes of death as well as of life—that with the increase of the living tissues there shall also be the excretion and destruction of dead, outgrown, and useless matters which have had their day and served their purpose. But *litera scripta manent.* There is a vast amount of this effete and worthless material in the literature of medicine,

and it is increasing rapidly. Our literature is, in fact, something like the inheritance of the golden dustman, but with this important difference—viz., that when the children raked a few shells or bits of bone from the dustman's heap, and, after stringing them together and playing with them a little while, threw them back, they did not thereby add to the bulk of the pile; whereas our preparers of compilations and compendiums, big and little, acknowledged or not, are continually increasing the collection, and for the most part with material which has been characterized as "superlatively middling, the quintessential extract of mediocrity." A large medical library is in itself discouraging to many inquirers, and I have become quite familiar with the peculiar expression of mingled surprise, awe, and despair which is apt to steal over the face of one not accustomed to such work when he first finds himself fairly in the presence of the mass of material which he wishes to examine for the purpose of completing his ideal bibliography of—let us say epilepsy, or excisions, or the functions of the liver.

Let such inquirers, as well as those who regret that they have no access to large libraries, and must therefore rely on the common text-books and current periodicals for bibliography, console themselves with the reflection that much the larger part of all of our literature which has any practical value belongs to the present century, and, indeed, will be found in the publications of the last twenty years.

There are a few books written prior to 1800 which every well-educated medical man should—I will not say read but—dip into, such as some of the works of Hippocrates and Galen, of Harvey and Hunter, of Morgagni and Sydenham; but this is to be done to learn their methods and style rather than their facts or theories, and by the great majority of physicians it can be done with much more profit in modern translations than in the originals. The really valuable part of the observations of these old masters has long ago become a part of the common stock, and the results are to be found in every text-book.

If, perchance, among the dusty folios there are stray golden grains yet ungleaned, remember that just in front are whole fields waiting the reaper. There is not, and has not been, any lack of men who have the taste and time to search the records of the past, and the man who has opportunities to make experiments or observations for himself wastes his time, to a certain extent, if he tries to do bibliographical work so long as he can get it done for him. He wishes to know whether this problem has been attacked before, and with what result—whether there are accounts of any other cases like the one he has in hand. In ninety-nine instances out of a hundred if the answers to these questions are not given in the current text-books or monographs it is not worth prolonged search by the original investigator. Yet he should know how to make this search, if only to enable him to direct others, and it is for this reason that a little acquaintance with bibliographical methods of work ought to be obtained by the student.

When a physician has observed (or thinks he has observed) a fact, or has evolved from his inner consciousness a theory which he wishes to examine by the light of medical literature, he is often very much at a loss to know how to begin, even when he has a large library accessible for the purpose.

The information he desires may be in the volume next his hand, but how is he to know that? And even when the usual subject-catalogue is placed before him he finds it very difficult to use it, especially when, as is often the case, he has by no means a well-defined idea as to what it is he wishes to look for. Upon the title-page of the Washington City Directory is printed the following aphorism, "To find a name you must know how to spell it." This has a very extensive application in medical bibliography. To find accounts of cases similar to your own rare case you must know what your own case is.

To return to the subject-catalogue. If it is a classed catalogue—a catalogue raisonnée—it will often seem to be a very blind guide to one who is not familiar with the classification and nomenclature adopted by the compiler. And certainly some of these classifications are very curious—reminding one of Heine's division of ideas into reasonable ideas, unreasonable ideas, and ideas covered with green leather. But if the inquirer has mastered the arrangement of the catalogue, it is two to one that it will not help him. It is a catalogue of the titles of books, but very often the title of a book gives very little information as to its contents, if, indeed, it is not actually misleading. Now, suppose the particular case he has in hand is one of a new-born infant having one leg much larger and longer than the other. He will find no book title relating to this. There may be a book in the library on diseases of the lymphatics which contains just what he wants, but unless he knows that his case is one affecting the lymphatics, he will hardly get the clue. There may also be in the library twenty papers, in as many different volumes of journals and transactions, the titles of which show that they probably relate to similar cases, but the titles of such papers do not appear in the catalogue.

It should also be observed that subject-catalogues may easily be put to improper uses, or thought to give more information than they really do. They are not bibliographies, but mechanical aids in bibliographical work.

You will perhaps pardon me for taking as an illustration the Index-Catalogue of the Library of the Surgeon-General's Office, in Washington, as being one with which I am familiar, and which I can venture to comment on without risk of its being thought that I wish to depreciate its value. Taking any given subject in medicine, it is possible for a fairly educated physician to obtain from this catalogue a large proportion of all the references which have any special value, and by so doing to save a vast amount of time and labour. On the other hand, he will find, when he comes to examine the books and articles referred to, that at least one-half of them are of no value so long as the other half are accessible, seeing that they are dilutions and dilatations, rehashes and summaries of the really original papers. If the seeker is in the library itself this does not cause a great waste of time, as he can rapidly examine and lay aside those that do not serve his purpose. But if he is using this catalogue in another library—say here in London, the case is different. It is highly improbable that he will find in any other collection all the books referred to, and then comes the annoyance of the doubt as to whether he may not be missing some very valuable paper. How is he to know whether or not Smith in his pamphlet on the functions of the pneumogastric has anticipated his own theory of its relations to enlarged tonsils? And in all such cases *omne ignotum est pro magnifico*. In a bibliography of the subject, prepared from the same material as the catalogue, he would either find no mention of Smith's paper, or, better still, a note that his paper is merely an abstract or compilation. The fact that he does not find Smith's book in the London library, nor any allusion to it in the best works on the subject, ought to induce him to ignore it altogether.

In proportion to the energy of the young writer, and his determination to not only note everything that has been written about his subject, but to carry out the golden rule of verifying all his references, he is apt to be led off from his direct research into the many attractive by-paths of quaint and curious speculation which he will find branching off on every side, and this danger must be guarded against, or he will find that he is wasting his time and energy in turning over chaff which has long ago been pretty thoroughly threshed and winnowed.

It is, however, no part of my present purpose to set forth the methods and principles of bibliography, it is sufficient to point out their importance, and to call attention to the point that a knowledge of how and where to find the record of a

fact is often of more practical use than a knowledge of the fact itself, just as we value an encyclopædia for occasional reference, and not for the purpose of reading through from cover to cover.

Instruction in the history and literature of medicine forms no part of the course of medical education in English and American schools, nor should I be disposed to recommend its introduction into the curriculum if it were to be based on French and German models, but it does seem possible to take a step in this direction which would be of great value; not only as a means of general culture, as teaching students how to think, but from a purely practical point of view, in teaching them how to use the implements of their profession to the best advantage—for books are properly compared to tools of which the index is the handle. Such instruction should be given in a library, just as chemistry should be taught in a laboratory. The way to learn history and bibliography is to make them—the best work of the instructor is to show his students how to make them.

In the absence of some instruction of this kind the student is liable to waste much time in bibliographical research. There has been much more done in this direction than many writers seem to suppose, and there are not many subjects in medicine which have not been treated from this point of view. Of course, all is not bibliography which pretends to be such, very many of the exhaustive and exhausting lists of references which are now so common in medical journal articles have been taken largely at secondhand, and thereby originate or perpetuate errors. It is well to avoid false pride in this matter. To overlook a reference is by no means discreditable—but a wrong reference, or an unwitting reference to the same thing twice gives a strong presumption of carelessness and secondhand work. Journal articles, however, and especially reports of cases, undergo strange transmogrifications sometimes, and I have watched this with interest in the case of a French or German paper, translated and condensed in the *London Record*, then appearing in abstract under the name of the translator in a leading journal, then translated again, with a few new circumstances, in a continental periodical, and finally perhaps reversed and appearing as an original contribution in the pages of the *Little Peddlington Medical Universe*.

In this connection it is well to remember that a mere accumulation of observations, no matter how great the number, does not constitute science, especially if these observations have been recorded under the influence of the same theories and in essentially similar conditions.

Science seeks the law which governs or explains the phenomena, and when this is found the records of isolated instances of its action usually become of small importance so far as that law is concerned. We care little now for the records of the chemical experiments of a century ago, and the many detailed accounts of the earlier cases of the use of ether or chloroform are of so little interest at the present time that it is not worth while to refer to them in a bibliography of the subject. And although much has been done towards classifying and indexing our medical records (more, in fact, than most physicians suppose), still, as Helmholtz points out, such knowledge as this hardly deserves the name of science, since it neither enables us to see the complete connection nor to predict the result under new conditions yet untried.

Do I seem to depreciate the value of the thoughts which our masters have left us, and which have furnished the foundations on which we build?—or to undervalue the importance of the great medical libraries in which are stored these thoughts?—or to speak slightly of the utility of the catalogues, and indexes, and bibliographies, without which such libraries are trackless and howling wildernesses? If so, I have said what I did not mean to say. The subject has been considered from the point of view of what used to be called the division of

labour, but which now I suppose should be called evolution and differentiation; and this has been done because "life is short and the art is long," with fair prospect of becoming longer. It is surely unnecessary for me to enter upon any panegyric of books or libraries. As Dr. Holmes says, "It is not necessary to maintain the direct practical utility of all kinds of learning. Our shelves contain many books which only a certain class of medical scholars will be likely to consult. There is a dead medical literature, and there is a live one. The dead is not all ancient; the live is not all modern. There is none, modern or ancient, which, if it has no living value for the student, will not teach him something by its autopsy. But it is with the live literature of his profession that the medical practitioner is first of all concerned."

In medicine, as in social science, we must depend for many facts upon the observation of conditions which occur very rarely, and which cannot be repeated at pleasure. I have already alluded to the importance of Nature's vivisections to the physiologist, and a record of a case written a century ago may be just the link that is needed to correlate the results of his experiments of yesterday with existing theories. The case which at first seems unique and inexplicable both receives and furnishes light when compared with ancient records.

A science of medicine, like other sciences, must depend upon the classification of facts, upon the comparison of cases alike in many respects, but differing somewhat either in their phenomena or in the environment. The great obstacle to the development of a science of medicine is the difficulty in ascertaining what cases are sufficiently similar to be comparable, which difficulty is in its turn largely due to insufficient and erroneous records of the phenomena observed. This defect in the records is largely due, first, to ignorance on the part of observers; second, to the want of proper means for precisely recording the phenomena; and, third, to the confused and faulty condition of our nomenclature and nosological classifications.

Let us consider each of these points briefly. Very, very few are the men who can by and for themselves see and describe the things that are before them. Just as it took thousands of years to produce a man who could see what now any one can see when shown him, that the star Alpha in Capricorn is really two separate stars, so we had to wait long before the man came who could see the difference between measles and scarlatina, and still longer for the one who could distinguish between typhus and typhoid. Said Plato, "He shall be as a god to me who can rightly divide and define." Men who have this faculty—the "Blick" of the Germans—we cannot produce directly by any system of education; they come, we know not when or why, "forming a small band, a mere understanding of whose thoughts and works is a test of our highest powers. A single English dramatist and a single English mathematician have probably equalled in scope and excellence of original work in their several fields all the like labours of their countrymen put together."[1]

But cannot we do something to increase the number of observers by telling them what to observe? It is probable that much may be accomplished in this direction provided that care be taken to limit the field. Manuals of "what to observe at the bedside and in the post-mortem room" are very well in their way, but can never be made to reach the great majority of the profession, nor would they be of much use if they did. If a few, a very few, distinct specific questions are brought to the attention of the general practitioner, he will often be on the alert for their answer. And it should be remembered that chance may present to the most obscure practitioner an opportunity for observation which the greatest master may never meet.

[1] Iles, Mathematics in Evolution. Pop. Sci. Monthly, 1876, ix. p. 207.

The great difficulty is to get such questions prepared. They must relate to matters that are just in the nebulous region between the known and unknown—to points not yet clear, but of which we know enough to make it probable that by observing in a definite direction they can be made clear; and to prepare them requires not only knowledge, but a certain reaching out beyond knowledge. It usually happens that the man who has this faculty strives to answer his questions himself, and no doubt he can usually do it better than another. But much can be done towards defining and marking out what we do not know, and this has been a powerful aid to the progress of physiology in recent years. I have had occasion to refer to this in speaking of Prof. Foster's work on physiology, in each section of which an attempt is made to separate that which may be considered as proved from that which is merely probable; and thus almost every page becomes suggestive of work to be done.

Another example of what I mean will be found in a paper on the collection of data at autopsies by Professor H. P. Bowditch, of Boston (Trans. Mass. Med.-Legal Soc. 1, 1880, p. 139). Taking the results of an investigation into the absolute and relative size of organs at different periods of life, and in connection with different morbid tendencies, recently published by Professor Beneke, of Warburg, Dr. Bowditch urges the securing as large a number as possible of such data, and selects certain of Professor Beneke's results for special inquiry; as, for instance, that "the cancerous diathesis is associated with a large and powerful heart, capacious arteries, but a relatively small pulmonary artery, small lungs, well developed bones and muscles, and tolerably abundant adipose tissue." It can hardly be doubted that those who read the papers of Professors Bowditch and Beneke will be induced to examine things which before would have had for them no interest, and, therefore, to make and record observations in pathological anatomy which otherwise would have been lost.

The second difficulty referred to—viz., the want of means for making accurate records—is one that is yearly growing less. It behooves us to be modest in our predictions as to what may be accomplished in the future towards the solution of our Sphinx's riddle. We see as through a glass darkly, and except through the glass in no wise; but, at least, we have made such progress that what we do see we can, to a great extent, so record that our successors yet unborn can also see, and it is owing to this fact that a part of the medical literature of the last quarter of the nineteenth century will be more valuable than all that has preceded it.

The word-pictures of disease traced by Hippocrates and Sydenham, or even those of Graves and Trousseau, interesting and valuable as they are, are not comparable with the records upon which the skilled clinical teacher of the present day relies. Yet how imperfect in many cases are even the best of these records as compared with what might be given with the resources which we have at our command. The temperature chart has done away with the errors which necessarily follow attempts to compare the memory of sensations perceived last week with the sensations of to-day—and the balance and the burette enable us to estimate with some approach to precision the tissue changes of our patients by the records of change in the excretions which they furnish; but we must still trust to our memory, or to the imperfect descriptions of what others remember, when we attempt to compare the results obtained on successive days by auscultation or percussion, although the phonograph and microphone strongly hint to us the possibility of either accurately reproducing the sounds of yesterday, or of translating them into visible signs, perhaps something like the dot and dash record of the telegraph code, which could then be given to the press, and so compared with each other by readers at the antipodes.

We are beginning to count the blood corpuscles, and to use photomicrography,

but we do not yet apply the latter process to the former so as to enable every reader to count for himself.

The connections of medicine with the physical sciences are yearly becoming closer, and the methods by which these sciences have been brought to their present condition are those by which progress has been, and is to be, made in therapeutics as well as in diagnosis or in physiological research. These methods turn mainly upon increasing the delicacy and accuracy of measurements; of expressing manifestations of force in terms of another force, or of dimension in space or time. The balance and the galvanometer, the microscope and the pendulum, the camera, the sphygmograph and the thermometer are some of the means by which investigators, at the bedside and in the laboratory, are seeking to obtain records which shall be independent of their own sensations or personal equations; which shall be taken and used as expressing not opinions, but facts; and with every addition to or improvement in these means of measurement and record, the field of observation widens, and new and more reliable materials are furnished for the application of logical and mathematical methods.

Upon the third difficulty which has been referred to, viz., our confused and defective terminology, I need not dwell. "Science," said Condillac, "is a language well made," and though this is far from being the whole truth, it is an important part of it. In examining medical reports and statistics, it is necessary to bear constantly in mind that to understand many terms you must know what the individual writer means by them. When, for example, we find in such statistics a certain number of deaths attributed to gastro-enteritis, or croup, or scrofula, we have to take into account the country, the period, and the individual author, in order to get even a fair presumption as to what is meant.

The three difficulties which have been referred to, although the most important, are by no means the only causes of the confusion and imperfection of our records.

Prominent among the minor troubles of the investigator are defective or misleading titles, and in behalf of the readers and bibliographers of the future I would appeal to authors, and more especially to editors, to pay more attention than many of them do to the matter of titles and indexes. The men to whom your papers are most important, and who will make the best use of them provided they know of their existence, are for the most part hard workers, busy men who have a right to demand that their literary table shall be provided with properly prepared materials and not with shapeless lumps.

The editors of Transactions of Societies, whether these are sent to journals or published in separate form, often commit numerous sins of omission in the matter of titles. The rule should be that every article which is worth printing is worth a distinct title, which should be as concise as a telegram, and be printed in a special type. If the author does not furnish such a title, it is the editor's business to make it; and he should not be satisfied with such headings as "Clinical Cases," "Difficult Labour," "A Remarkable Tumour," "Case of Wound, with Remarks." The four rules for the preparation of an article for a journal will then be: 1. Have something to say. 2. Say it. 3. Stop as soon as you have said it. 4. Give the paper a proper title.

Some societies and editors do not seem to appreciate fully their responsibility for the articles which they accept for publication; a responsibility which cannot be altogether avoided by any formal declaration disclaiming it. This is due to the fact that while the merits of a paper can usually be determined by examination, this is by no means always the case. In every country there are writers and speakers whose statements are received with very great distrust by those best acquainted with them. Supposing these statements to be true, the papers

would be of much interest and importance; but the editor should remember that a certain number of readers, and especially those in foreign countries, have no clue to the character of the author beyond the fact that they find his works in good company. In medical literature, as in other departments, we find books and papers from men who are either constitutionally incapable of telling the simple, literal truth as to their observations and experiments, although they may not write with fixed intentions to deceive, or from men who seek to advertise themselves by deliberate falsehoods as to the results of their practice. Such men are usually appreciated at their true value in their immediate neighbourhood, and find it necessary to send their communications to distant journals and societies in order to secure publication.

I presume that you are all familiar with the peculiar feeling of distrust which is roused by too complete an explanation. The report of a case in which every symptom observed, and the effect of every remedy given is fully accounted for, and in which no residual unexplained phenomena appear, is usually suspicious, for it implies either superficial observation or suppression, or distortion of some of the facts. A diagrammatic representation is usually much plainer than a good photograph, but also of much less value as a basis for further work.

No fact is more familiar to this audience than the vast extent of the field of the science of to-day—so vast that few may hope to master more than a small part of it, and yet so closely connected that even the small part cannot be fully grasped without some acquaintance with a much wider field.

But little over a hundred years ago, Haller, in Göttingen, was professor of anatomy, botany, physiology, surgery, and obstetrics, and lecturer on medical jurisprudence. At the same time he was writing one review a week, and summing up existing medical science in his "Bibliotheca." To-day any one of these branches requires all the time of the most energetic and learned of our contemporaries; but, on the other hand, the well-educated medical graduate of to-day could give Haller valuable instruction in each of the branches of which he was professor. It is also true, as I have pointed out, that our actual progress is by no means in proportion to the work done, nor as great as these merely quantitative statements would seem to make it.

Science has been termed "the topography of ignorance." From a few elevated points we triangulate vast spaces, inclosing infinite unknown details. We cast the lead, and draw up a little sand from abysses we shall never reach with our dredges.

If it is true that we understand ourselves but imperfectly in health, it is more signally manifest in disease, where natural actions imperfectly understood, disturbed in an obscure way by half-seen causes, are creeping and winding along in the dark towards their destined issue, sometimes using our remedies as safe stepping-stones, occasionally, it may be, stumbling over them as obstacles.[1]

In days of old, when the profession of medicine, or of a single medical specialty, was an inheritance in certain families, a large part of their knowledge and the efficiency of their remedies was thought to depend upon these being kept a profound mystery. Among the precepts of magic there was no more significant one than that which declared that the communication of the formula destroyed its power, and that hence attempts to reveal the secret must always fail.

We have changed all that. Every physician hastens to publish his discoveries and special knowledge, and a good many do the same by that which is not special, or which is not knowledge. For the individual, in a degree—for the nation or the race in a much greater degree—the literature produced is the most endur-

[1] Border Lines of Knowledge, etc., by O. W. Holmes, Boston, 1862, pp. 7, 8.

ing memorial. The whole result of civilization has been cynically defined as being roughly, "Three hundred million Chinese, two hundred million natives of India, two hundred million Europeans and North Americans, and a miscellaneous hundred million or two of Central Asians, Malays, South Sea Islanders, etc., and over and above all the rest the Library of the British Museum. This is the net result of an indefinitely long struggle between the forces of men and the weights of various kinds in the attempt to move which these forces display themselves."[1]

And thus in our great medical libraries each of the folios or quaint little black-letter pamphlets which mark the first two centuries of printing, or of the cheap and dirty volumes of more modern days, with their scrofulous paper and abominable typography, represents to a great extent the life of one of our profession and the fruit of his labours, and it is by the fruit that we know him.

After stating that modern physicists have concluded that the sun is going out, that the earth is falling into the sun, and therefore that it and all things in it will be either fried or frozen, Professor Clifford concludes that "our interest lies so much with the past as may serve to guide our actions in the present, and with so much of the future as we may hope will be affected by our actions now. Beyond that we do not know and ought not to care. Does this seem to say, Let us eat and drink, for to-morrow we die? Not so, but rather, Let us take hands and help, for this day we are alive together." To this I join a verse from the Talmud, which will remind you of the first aphorism of Hippocrates, and is none the worse for that: "The day is short, and work is great—the reward is also great, and the master presses. It is not incumbent on thee to complete the work, but thou must not therefore cease from it."—*Lancet*, Aug. 13, 1881.

TRANSLATION OF AN ADDRESS ON THE GERM THEORY.

Delivered at the International Medical Congress, London, August, 1881.

By PROFESSOR PASTEUR.

Gentlemen: I had no intention of addressing this admirable Congress, which brings together the most eminent medical men in the world, and the great success of which does so much credit to its principal organizer, Mr. Mac Cormac. The good-will of your esteemed President has decided otherwise. How could one, in fact, resist the sympathetic words of that eminent man whose goodness of heart is associated in no small degree with great oratorical ability? Two motives have brought me to London. The first was to gain instruction, to profit by your learned discussions; and the second was to ascertain the place now occupied in medicine and surgery by the germ theory. Certainly I shall return to Paris well satisfied. During the past week I have learned much. I carry away with me the conviction that the English people are a great people, and as for the influence of the new doctrine, I have been not only struck by the progress it has made, but by its triumph. I should be guilty of ingratitude and of false modesty if I did not accept the welcome I have received among you and in English society as a mark of homage paid to my labours during the past five-and-twenty years upon the nature of ferments—their life and their nutrition, their preparation in a pure state by the introduction of organisms (*ensemencement*) under natural and artificial conditions — labours which have established the principles and the methods of microbie (microbism), if the expression is allowable. Your cordial

[1] Liberty, Equality, and Fraternity, by James Fitz James Stephen, N. Y. 1873, p. 178.

welcome has revived within me the lively feeling of satisfaction I experienced when your great surgeon Lister declared that my publication in 1857 on milk fermentation had inspired him with his first ideas on his valuable surgical method. You have reawakened the pleasure I felt when our eminent physician Dr. Davaine declared that his labours upon charbon (splenic fever or malignant pustule) had been suggested by my studies on butyric fermentation and the vibrion which is characteristic of it. Gentlemen, I am happy to be able to thank you by bringing to your notice a new advance in the study of microbie as applied to the prevention of transmissible diseases—diseases which for the most part are fraught with terrible consequences, both for man and domestic animals. The subject of my communication is vaccination in relation to chicken cholera and splenic fever, and a statement of the method by which we have arrived at these results—a method the fruitfulness of which inspires me with boundless anticipations. Before discussing the question of splenic fever vaccine, which is the most important, permit me to recall the results of my investigations of chicken cholera. It is through this inquiry that new and highly important principles have been introduced into science concerning the virus or contagious quality of transmissible diseases. More than once in what I am about to say I shall employ the expression virus-culture, as formerly, in my investigations on fermentation, I used the expressions, the culture of milk ferment, the culture of the butyric vibrion, etc.

Let us take, then, a fowl which is about to die of chicken cholera, and let us dip the end of a delicate glass rod in the blood of the fowl with the usual precautions, upon which I need not here dwell. Let us then touch with this charged point some *bouillon de poule*, very clear, but first of all rendered sterile under a temperature of about 115° Centigrade, and under conditions in which neither the outer air nor the vases employed can introduce exterior germs—those germs which are in the air, or on the surface of all objects. In a short time, if the little culture vase is placed in a temperature of 25° to 35°, you will see the liquid become turbid, and full of tiny microbes, shaped like the figure 8, but often so small that under a high magnifying power they appear like points. Take from this vase a drop as small as you please, no more than can be carried on the point of a glass rod as sharp as a needle, and touch with this point a fresh quantity of sterilized *bouillon de poule* placed in a second vase, and the same phenomenon is produced. You deal in the same way with a third culture vase, with a fourth, and so on to a hundred, or even a thousand, and invariably within a few hours the culture liquid becomes turbid and filled with the same minute organisms. At the end of two or three days' exposure to a temperature of about 30° C. the thickness of the liquid disappears, and a sediment is formed at the bottom of the vase. This signifies that the development of the minute organism has ceased—in other words, all the little points which caused the turbid appearance of the liquid have fallen to the bottom of the vase, and things will remain in this condition for a longer or shorter time, for months even, without either the liquid or the deposit undergoing any visible modification, inasmuch as we have taken care to exclude the germs of the atmosphere. A little stopper of cotton sifts the air which enters or issues from the vase through changes of temperature. Let us take one of our series of culture preparations—the hundredth or the thousandth, for instance—and compare it in respect to its virulence with the blood of a fowl which has died of cholera; in other words, let us inoculate under the skin ten fowls, for instance, each separately with a tiny drop of infectious blood, and ten others with a similar quantity of the liquid in which the deposit has first been shaken up. Strange to say, the latter ten fowls will die as quickly and with the same symptoms as the former ten; the blood of all will be found to contain after death the same minute infectious organisms. This equality, so to speak, in the virulence both of the

culture preparation and of the blood is due to an apparently futile circumstance. I have made a hundred culture preparations—at least, I have understood that this was done—without leaving any considerable interval between the impregnations. Well, here we have the cause of the equality in the virulence. Let us now repeat exactly our successive cultures with this single difference, that we pass from one culture to that which follows it—from the hundredth to, say, the hundred and first, at intervals of a fortnight, a month, two months, three months, or ten months. If, now, we compare the virulence of the successive cultures, a great change will be observed. It will be readily seen from an inoculation of a series of ten fowls that the virulence of one culture differs from that of the blood and from that of a preceding culture when a sufficiently long interval elapses between the impregnation of one culture with the microbe of the preceding. More than that, we may recognize by this mode of observation that it is possible to prepare cultures of varying degrees of virulence. One preparation will kill eight fowls out of ten, another five out of ten, another one out of ten, another none at all, although the microbe may still be cultivated. In fact, what is no less strange, if you take each of these cultures of attenuated virulence as a point of departure in the preparation of successive cultures and without appreciable interval in the impregnation, the whole series of these cultures will reproduce the attenuated virulence of that which has served as the starting point. Similarly, where the virulence is null it produces no effect. How, then, it may be asked, are the effects of these attenuating virulences revealed in the fowls? They are revealed by a local disorder, by a morbid modification more or less profound in a muscle, if it is a muscle which has been inoculated with the virus. The muscle is filled with microbes which are easily recognized because the attenuated microbes have almost the bulk, the form, and the appearance of the most virulent microbes. But why is not the local disorder followed by death? For the moment let us answer by a statement of facts. They are these: the local disorder ceases of itself more or less speedily, the microbe is absorbed and digested, if one may say so, and little by little the muscle regains its normal condition. Then the disease has disappeared. When we inoculate with the microbe the virulence of which is null there is not even local disorder, the *naturæ medicatrix* carries it off at once, and here, indeed, we see the influence of the resistance of life, since this microbe, the virulence of which is null, multiplies itself. A little further, and we touch the principle of vaccination. When the fowls have been rendered sufficiently ill by the attenuated virus which the vital resistance has arrested in its development, they will, when inoculated with virulent virus, suffer no evil effects, or only effects of a passing character. In fact, they no longer die from the mortal virus, and for a time sufficiently long, which in some cases may exceed a year, chicken cholera cannot touch them, especially under the ordinary conditions of contagion which exist in fowl-houses. At this critical point of our manipulation—that is to say, in this interval of time which we have placed between two cultures, and which causes the attenuation—what occurs? I shall show you that in this interval the agent which intervenes is the oxygen of the air. Nothing more easily admits of proof. Let us produce a culture in a tube containing very little air, and close this tube with an enameller's lamp. The microbe in developing itself will speedily take all the oxygen of the tube and of the liquid, after which it will be quite free from contact with oxygen. In this case it does not appear that the microbe becomes appreciably attenuated, even after a great lapse of time. The oxygen of the air, then, would seem to be a possible modifying agent of the virulence of the microbe of chicken cholera—that is to say, it may modify more or less the facility of its development in the body of animals. May we not be here in presence of a general law applicable

to all kinds of virus? What benefits may not be the result? We may hope to discover in this way the vaccine of all virulent diseases; and what is more natural than to begin our investigation of the vaccine of what we in French call charbon, what you in England call splenic fever, and what in Russia is known as the Siberian pest, and in Germany as the Milzbrand. In this new investigation I have had the assistance of two devoted young *savants*—MM. Chamberland and Roux. At the outset we were met by a difficulty. Among the inferior organisms, all do not resolve themselves into those corpuscle germs which I was the first to point out as one of the forms of their possible development. Many infectious microbes do not resolve themselves in their cultures into corpuscle germs. Such is equally the case with beer yeast, which we do not see develop itself usually in breweries, for instance, except by a sort of scissiparity. One cell makes two or more, which form themselves in wreaths; the cells become detached, and the process recommences. In these cells real germs are not usually seen. The microbe of chicken cholera and many others behave in this way, so much so that the cultures of this microbe, although they may last for months without losing their power of fresh cultivation, perish finally like beer yeast which has exhausted all its aliments. The anthracoid microbe in artificial cultures behaves very differently. In the blood of animals, as in cultures, it is found in translucid filaments more or less segmented. This blood or these cultures freely exposed to air, instead of continuing according to the first mode of generation, show at the end of forty-eight hours corpuscle germs distributed in series more or less regular along the filaments. All around those corpuscles matter is absorbed, as I have represented it formerly in one of the plates of my work on the disease of silkworms. Little by little all connection between them disappears, and presently they are reduced to nothing more than germ dust. If you make these corpuscles germinate, the new culture reproduces the virulence peculiar to the threadly form which has produced these corpuscles, and this result is seen even after a long exposure of these germs to contact with air. Recently we discovered them in pits in which animals dead of splenic fever had been buried for twelve years, and their culture was as virulent as that from the blood of an animal recently dead. Here I regret extremely to be obliged to shorten my remarks. I should have had much pleasure in demonstrating that the anthracoid germs in the earth of pits in which animals have been buried are brought to the surface by earthworms, and that in this fact we may find the whole etiology of disease, inasmuch as the animals swallow these germs with their food. A great difficulty presents itself when we attempt to apply our method of attenuation by the oxygen of the air to the anthracoid microbes. The virulence establishing itself very quickly, often after four-and-twenty hours in an anthracoid germ which escapes the action of the air, it was impossible to think of discovering the vaccine of splenic fever in the conditions which had yielded that of chicken cholera. But was there, after all, reason to be discouraged? Certainly not; in fact, if you observe closely, you will find that there is no real difference between the mode of the generation of the anthracoid germ by scission and that of chicken cholera. We had therefore reason to hope that we might overcome the difficulty which stopped us by endeavouring to prevent the anthracoid microbe from producing corpuscle germs and to keep it in this condition in contact with oxygen for days, and weeks, and months. The experiment fortunately succeeded. In the ineffective (*neutre*) *bouillon de poule* the anthracoid microbe is no longer cultivable at 45° C. Its culture, however, is easy at 42° or 43°, but in these conditions the microbe yields no spores. Consequently it is possible to maintain in contact with the pure air at 42° or 43° a *mycélienne* culture of bacteria entirely free of germs. Then appear the very remarkable results which follow. In a month or six weeks the culture dies—that

is to say, if one impregnates with it fresh *bouillon*, the latter is completely sterile. Up till that time life exists in the vase exposed to air and heat. If we examine the virulence of the culture at the end of two days, four days, six days, eight days, etc., it will be found that long before the death of the culture the microbe has lost all virulence, although still cultivable. Before this period it is found that the culture presents a series of attenuated virulences. Everything is similar to what happens in respect to the microbe in chicken cholera. Besides, each of these conditions of attenuated virulence may be reproduced by culture; in fact, since the charbon does not operate a second time (*ne récidive pas*), each of our attenuated anthracoid microbes constitutes for the superior microbe a vaccine—that is to say, a virus capable of producing a milder disease. Here, then, we have a method of preparing the vaccine of splenic fever. You will see presently the practical importance of this result, but what interests us more particularly is to observe that we have here a proof that we are in possession of a general method of preparing virus vaccine based upon the action of the oxygen and the air—that is to say, of a cosmic force existing everywhere on the surface of the globe. I regret to be unable from want of time to show you that all these attenuated forms of virus may very easily, by a physiological artifice, be made to recover their original maximum virulence. The method I have just explained of obtaining the vaccine of splenic fever was no sooner made known than it was very extensively employed to prevent the splenic affection. In France we lose every year by splenic fever animals of the value of 20,000,000f. I was asked to give a public demonstration of the results already mentioned. This experiment I may relate in a few words. Fifty sheep were placed at my disposition, of which twenty-five were vaccinated. A fortnight afterwards the fifty sheep were inoculated with the most virulent anthracoid microbe. The twenty-five vaccinated sheep resisted the infection; the twenty-five unvaccinated died of splenic fever within fifty hours. Since that time my energies have been taxed to meet the demands of farmers for supplies of this vaccine. In the space of fifteen days we have vaccinated in the departments surrounding Paris more than 20,000 sheep and a large number of cattle and horses. If I were not pressed for time I should bring to your notice two other kinds of virus attenuated by similar means. These experiments will be communicated by and by to the public. I cannot conclude, gentlemen, without expressing the great pleasure I feel at the thought that it is as a member of an international medical congress assembled in England that I make known the most recent results of vaccination upon a disease more terrible, perhaps, for domestic animals than smallpox is for man. I have given to vaccination an extension which science, I hope, will accept as homage paid to the merit and to the immense services tendered by one of the greatest men of England, Jenner. What a pleasure for me to do honour to this immortal name in this noble and hospitable city of London!—*Lancet*, Aug. 13, 1881.

ADDRESS ON THE CHANGES WHICH SURGERY HAS UNDERGONE DURING THE LAST TEN YEARS.

Delivered at the Meeting of the International Medical Congress, London, 1881.

By PROFESSOR VOLKMANN.

GENTLEMEN: It is with hesitation and only in accordance with the wish of my London friends that I have undertaken to speak before such a distinguished assembly, and to make an attempt to sketch a hasty picture of the changes which

surgery has experienced in the last fifteen or even ten years. And I trust that in this attempt I may rely on your special indulgence; for great and unparalleled in the history of medical science have been those changes. Problems thousands of years old have been solved, or are at any rate approaching a sure solution; the desires of our fathers have been fulfilled beyond their hope or expectation. But all our practice and theories have been also fundamentally altered. The position of our science, the position that we ourselves occupy towards invalids, have become entirely different. And this revolution, in the midst of which we still stand, although the first wave has exhausted itself, has been called forth by the one incontestable fact, that all those countless and disastrous disturbances by which the wounds and hence also the life of those operated on or injured are threatened are only the consequences of particular processes of decomposition of the animal fluids, brought about by the intrusion of lower organisms.

For with the recognition of the manner of their origin, and with the knowledge of their nature, we have also gained the power of preventing these disturbances. Conscious of our aim, we are able so to act as to prevent the evil influences of these micro-organisms. The fate of countless invalids is from henceforth placed in our hands. In the place of blind chance, in the place of fortune or misfortune, which formerly played so great a part in the work of the practical surgeon, there now appear, in quite a different degree, knowledge and ignorance, capability and inability, care and carelessness. But a short time ago the surgeon, when he had, according to the rules of his art, made a wound, was like the husbandman who, when he has sown his field, patiently awaits the harvest, and accepts it as it may turn out, powerlessly exposed to the elements which may bring him rain and sunshine, storm and hail. Now he is the manufacturer from whom we expect good wares. By rescuing from the domain of chance the results of our labours, as far as they depend on operations and the treatment of wounds—and this will always remain the chief and especial work of surgery—the antiseptic method has elevated surgery to the rank of the latest experimental science.

Never has a discovery been made in surgery which has even approached this in its benefits to humanity in general. Many thousands of human beings have in the short space of time that has elapsed since then preserved life and limbs, and been spared pain and a long confinement to a sick-bed; and millions will yet share in these benefits, for the principles of the antiseptic treatment of wounds will *never* again be abandoned as long as the whole of our knowledge is not lost, no matter how our art or the points of attack may change. Perhaps we may some day succeed in treating injured limbs simply in heated or filtered air, or may learn to strengthen the living power of resistance of the tissues and organs so that they can of themselves resist the action of those invisible enemies. The protection afforded by vaccination makes it appear possible that in this direction also new paths are opening out to us.

England may be proud that it was one of her sons whose name is inseparably connected with this the greatest advance that surgery has ever made. Without envy other nations may yield her the crown. For the long and silent labour which made the ripening of the seed possible, which we now reap so quickly and fully, was quite international, and especially France and Germany took part in it in an equal degree. Nor did any one more fully recognize the importance of those who worked before him than Mr. Joseph Lister.

Moreover, it has happened with the discovery of the antiseptic method as with so many great discoveries; years, sometimes centuries, prepare them, they are in the air as it were, until one day, to the surprise of mankind, they suddenly seem to fall down from heaven. Nothing is more instructive than to reflect the

light of our present knowledge on the endeavours of past times, and to see how since the beginning of this century the most heterogeneous opinions have been held concerning the treatment of wounds; how methods apparently the most opposite have been employed, in all of which we may, however, now recognize a certain value—how wounds were treated with ice, with the cautery iron, with alcohol; how they were ventilated or hermetically closed; how the subcutaneous mode of operation was resorted to without deciding the question why the air coming into contact with the injured tissues of the body in one series of cases causes such deleterious results, in others appears perfectly harmless; how, seeking without any firm leading principle, experimenting and often abandoning one's self to the rudest empiricism, yet the goal was approached nearer and nearer—so near, that we might suppose it was almost possible even then to grasp the truth, and that every one ought to have recognized it, and then, when daylight is approaching, and even when the chief problem has been solved, that then the complete failure of our art was shown by the adoption of the open treatment of wounds.

I consider the directions given by Stromeyer and Pirogoff, especially for gunshot wounds, and in their time perfectly justified, never to probe a fresh wound, is here carried out to actual nihilism, and, to our shame, proofs are given that with passive looking on and the renunciation of quick healing by first intention, with good cicatrizing, the results were on an average better than in all active methods of treatment practised up to that time. Gentlemen, I wish not to be misunderstood. I do not mean that open treatment of wounds which is at this day employed in the most efficacious manner in combination with strictly antiseptic measures where the locality or serious disturbances already caused in the wound do not permit of the usual appliances, or where after an operation performed with all the means of protection which modern surgery affords us, we may so safely count upon a healing by first intention that further measures seem unnecessary. I will also point out that the German surgeon who first employed the open treatment of wounds and gave it its name, and achieved excellent results with it—Burow—introduced in connexion with it a remedy which we now know as one of the strongest antiseptics—*i. e.*, the argilla acetica. But I speak of that open treatment of wounds which, rejecting all the attainments of modern surgery, was so emphatically lauded as a special method to be preferred to all other modes of treatment.

Gentlemen, in speaking of the open treatment of wounds, I have touched on the opposition which the new doctrine at first met with on so many sides. You all know well how violent this was. Even in science great changes cannot be brought about without injury to numerous personal interests. But I insist that nowadays no serious opposition exists, that there is no surgeon who would dare at the operating-table or in the treatment of the wounded quite to renounce antiseptic methods; who calmly and with a good conscience would tread the paths in which fifteen years ago we all followed like cattle. Even the most obstinate have had to give way to the great principles of the new era, and often in a much higher degree than they acknowledged to themselves or others; the necessity not only of the disinfection of hands, sponges, instruments, and bandages, but also the necessity of a primary disinfection of fresh wounds, is, I believe, universally acknowledged.

In this respect, then, the time that we have lived through stands alone. In a period of a few years a new revolutionizing doctrine and a new difficult method of treatment which increases to the utmost the responsibility of the surgeon treating the case has celebrated its triumph through the whole of the educated world.

I hope you will think it natural if I, in the attempt to sketch the most essential

points by which modern surgery differs from that of past times, place in the first ranks the immense diminution of mortality after serious operations and severe injuries. It seems natural to prove the advances made in this respect by comparing the mortality under the old surgery with that of the present day. But in spite of all trouble and labour, the only result of such attempts is the conviction that our present results are incomparably better. How great used to be the average losses caused by important operations and serious injuries we shall never learn. One thing is certain, that they were much greater even than most of the available calculations made them appear. But this second fact is just as certain; that the calculation of averages had at that time scarcely any value, because the mortality was subject at different times, in different hospitals, and under different surgeons, to the most enormous variations, as a glance into the celebrated work of Malgaigne will show. Average numbers can only be of value when the variations take place within certain fixed limits. The high tables of mortality of old surgery have been obtained by the addition of series of cases which differed from one another fivefold or tenfold; and as soon as they were applied by the sick-bed or in the judgment of the results of treatment, they let any result appear satisfactory, and, if fatal, excused it.

We may therefore say without exaggeration that the old statistics of surgery were of no use, but only did harm, and we may oppose to their sad, unsatisfactory figures the simple demand that *no* person should die of an injury unless its severity directly threatens his life, and that no person injured or operated on should perish through a secondary inflammatory disturbance developed from the wound. Every loss of this kind may be traced to some mistake on our part.

But we, who have ourselves groaned beneath the weight of the old surgery, and have now made the new our own, possess comparative statistics that are infinitely more valuable to us than all those that our opponents have held up against us, statistics which we have fought for in the last few years. The time is long enough to have excluded all the old theories of chance. During ten years it is not possible to be always lucky, always to have good cards, as even Pirogoff still ventured to assert of certain privileged surgeons.

And if we compare our former with our present results we find a difference like that between day and night, the feeling of a great victory after long and severe defeat; and never do I feel more solemnly and gratefully inclined to Providence, who has permitted me to live to see this blessed change, than when I make this comparison.

Professor Nussbaum has drawn this comparison between then and now in his clinic in clear traits and with manly candour; he has shown that for forty years, under his own direction as well as under that of his predecessors, among whom was Stromeyer, fatal wound diseases raged, that nearly all patients with compound fractures died of them, and that even those with the slightest injuries often succumbed to them; that erysipelas and abscess were matters of daily occurrence, that during the latter years hospital gangrene also appeared and attacked the dreadful number of eighty per cent. of all wounds and sores, that he stood helpless and powerless before all these conditions, and that at one step, after the general introduction of the antiseptic method, all this ceased, and instead, even after great operations, healing by first intention was introduced as an entirely new result.

I have also tried already at the third German Congress of Surgeons to draw a similar comparison; it is not essentially different from Nussbaum's. Since then many years have elapsed; and in all countries, but perhaps most of all in Germany, a great mass of experience has been collected, and it might perhaps be now possible by a comparison of large numbers to discover with some amount of

certainty what is really the chief question; how far we have got in the control of accidental wound diseases; what dependence is to be placed on our present technical aids; where and how often they leave us in the lurch, and where the limits to our present power are placed. Time and place do not permit me to make this attempt, and perhaps it would be beyond my power. But, with your permission, I will lay before you some data bearing on these questions, and if I take these chiefly from my own experience, you may be convinced that this is only because these are most accessible to me, and because I can myself vouch for their accuracy. For the satisfaction of personal vanity I should probably have chosen another subject than this which shows what can be achieved by a method in the invention of which I had no share, and should have brought forward other results than those which were only achieved by submitting entirely during the first years to the prescriptions of Joseph Lister, and resisting every temptation to do better and more simply than he. Most of the unfavourable judgments passed on this method are due to the fact that surgeons who had not yet learnt to experiment *with it* already made *it* the subject of their experiments.

Two examples will suffice, compound fractures and major amputations. The mortality after compound fracture had, during the long labours of my predecessor as well as during my own, reached the sad height of forty per cent. When I adopted the antiseptic treatment of wounds my last twelve patients, with compound fracture of the leg, had all died of pyæmia or septicæmia. From that time up to the present day I have treated one after another 135 compound fractures, and not a single patient has succumbed to either of these accidental wound diseases; 133 were cured, two died, one of fat-embolism of the lungs, during the first few hours, and one, a drunkard, of delirium tremens.

For amputations one assertion will almost suffice, which I beg you to regard seriously; I now cure every year more cases of amputation of the thigh than during all the rest of my labours before the introduction of the antiseptic method. The number of the amputations of the larger limbs which I have undertaken during the last few years amounts to more than 400. If I subtract those cases where death did certainly not result in consequence of the operation, but independently of this from some other serious complication, there results a mortality of four to five per cent., and the same number, as far as I can discover from the communications before me, was obtained in the other German hospitals in which antiseptic surgery is practised in full strength.

My friend Dr. Schede has made a calculation which will, I believe, interest you. He has studied the reports of a great number of amputation cases of the old period, so that he was able to distinguish those in which death resulted from pyæmia or septicæmia—those therefore which we now save—and the mortality of the remaining cases also amounted to about five per cent. In these four to five per cents., besides those fatal cases caused by mistakes in treatment, those are also included which in amputations after serious injuries must be laid to the door of the shock caused by the previous serious loss of blood. We may therefore declare that amputation of the larger limbs has been almost absolutely free from danger—at any rate, much less dangerous than many small operations were formerly, whose mortality was never discussed. After extirpation of sebaceous cysts in the head, occasionally one or other of those operated on perished; and a by no means small part came off only with their lives. It is true, I must not disguise the fact, that my numbers have been slighted. It has been said that I kept back the unfavourable cases, and thus improve my figures of mortality. Quite lately a French colleague of mine declared that in France it would not be possible to agree to my sort of statistics!

But, gentlemen, what do we want? Do we want to count the amputated

limbs like fallen apples, which are picked up from under the trees? Or do we want to gain figures that answer questions which have a practical importance? If we want to know how dangerous *per se* is amputation at the upper part of the thigh, we cannot learn this from the patients who already suffer from severe septicæmia, or who, besides, suffer from severe brain injuries; and if we want to know how dangerous it is to remove *one* thigh, we cannot make use of cases where both have been amputated. But I will not complicate these statistics in order, as my opponents say, to contrast the most favourable with the most conceivably unfavourable cases. During the time of which I speak 57 large limbs were amputated—lost according to common parlance—because of acute progressive septic processes, chiefly patients with severe open fractures, wounds of the joints, and laceration of the soft parts, who were brought to us too late from the country, cases of embolic mortification and large putrid abscesses. All patients in whom signs of mortification had shown themselves, were not counted. Of these 57 amputated cases, in which, in almost half the cases, the operation concerned the thigh, fully 70 per cent. were cured. A mortality of 30 per cent. is lower than I formerly attained to in cases of primary amputation. It shows best how erroneous are the views of those who contend that antiseptic surgery can only attain results in prophylaxis. Far more rapidly than by means of figures and a survey of successes, we can attain to a comprehension of the enormous advance achieved by modern surgery when we see what is actually done in the domain of operations—what is held permissible, and what is done and risked daily without danger to the patient. For this it is not needful to be a surgeon, not even a doctor, in order to testify to the changes that have come about. Operations are now conducted which fifteen years ago would have been regarded as madness, or as crimes. And often it is the younger doctors who have not had much experience of independent practice who have this advantage over their more venerable colleagues—that they have faith in the infallibility of the antiseptic principle, and have a courage that has not yet been daunted by ill-success; and it is they who venture upon operations of which formerly the most daring surgeons did not think, and attain results concerning which these shake their heads.

But do you think, gentlemen, that one of us older ones could so easily take up the idea of Ogston's operation? For this one must be young, and have grown up in the new thoughts and surroundings, and only know the sad experiences of older surgery as a legend. It is far from me to wish to discuss the value of the operation for the cure of genu valgum. I only wish to show from what one does not shrink. It will always remain of historical interest that, a few years after a great war, in which we were obliged to let the greater number of our wounded die in whom a ball had pierced the knee-joint capsule without hurting the bones; we dared to saw off in the dark within the joint a condyle, leaving blood and sawdust behind. Further, it would be easy to produce numbers of as apparently venturesome operations of which no one doubts the necessity or that they should be permitted.

For a large enchondroma in the costal pleura that occupied the left wall of the thorax, Professor Fischer removed a large piece of the chest-wall and ribs, so that the heart and lungs were exposed and an opening as large as a child's head was made, and yet the patient was able to be discharged from the hospital after four weeks.

In the case of a large echinococcus of the liver, which in front and at the side was covered with thick layers of liver tissue, and which projected into the thoracic cavity after resection of the seventh rib, I opened the healthy pleural cavity, which was free from adhesions. The thorax was freely open, the thin diaphragm cut into, the echinococcus sac opened, the animal bladder extracted *in toto*, and

the patient recovered without complication. A similar operation, with like results, was conducted by Mr. Israel of Berlin. Mr. Hahn, also of Berlin, in two cases of wandering kidney, where the mobility and the discomfort induced thereby had attained an unusually high degree, drew out the organ in question through a large wound in the loin, and sewed it into the same. Both patients recovered and lost their pain. The opening of the joints seems a most innocent performance. Hips and knees are cut open in order, in a case of luxation, to search for the obstacle which opposes itself to reduction, in order to suture the ruptured tissues, and in obscure symptoms to clear up the diagnosis *in vivo* by means of autopsy. More than two hundred times I alone, without in one instance bad results following, have incised, drained, and washed out diseased knee-joints without exciting suppuration.

Bone and marrow tissue will submit to every kind of treatment without putrefying and without suppuration. Orthopædics celebrated their greatest triumphs in the saving and straightening of limbs. We read that Mr. McEwen alone has performed 835 osteotomies, of which 827 were healed without suppuration, and he lost no patient from the operation itself or from its consequences. To facilitate the healing of empyemic cavities, Mr. Schneider resected from six to nine ribs, and even also the clavicle. In very painful cases of syphilitic hyperostosis I myself have repeatedly exposed the tibia in its whole length, and knocked off with a chisel a whole handful of bone shavings. The cure was rapid; the pains disappeared. The old opinion that in cases of strumous suppuration wounds heal less well has been set aside—the largest scrofulous abscesses, even if they come from the spinal column, can be opened not only without danger, but often healed in a brief time through a special kind of first intention. The feat of excision of part of the stomach has excited the whole world, perhaps to excess, since the same operation has been already effected upon the intestines with the happiest results. Weak, paralyzed persons, who usually are anxiously shielded from every injury, have had their main nervous system exposed, to subject it to thorough manipulation.

And now leave the writings and let us realize the teachings of the greatest masters of the first half of this century, of those who were equally great as doctors and as men; for instance, Sir Astley Cooper, whom we may regard as the representative of the highest development of the older surgeons, and you will be convinced that their firm and leading principle, the moral standpoint on which they stood was this, that every operation is dangerous, and should if possible be avoided. There is not one of us who, led by B. von Langenbeck, did not stand on the same ground during the great wars of 1866 and 1870, in his treatment of the wounded. Only since 1870 the new views have forcibly made their way.

Cast a glance at the surgical clinics of hospitals, where these are conducted by men who have willingly given up the hardly won results of their own work, and have entirely adopted the knowledge and power of our new art, and here too you will see the greatest changes; cases of operations and wounds without fever and pain, a happy result being regarded as certain; no traces of blood and wound secretions on the bandages, let alone on the body and bed linen; disappearance of that incessant activity of untrustworthy and senseless assistants, the continuous putting on and taking off of compresses and cataplasms, the regular change of bandages repeated every morning and evening. It would thus appear that in association with anæsthetics and the bloodless method, antiseptic surgery has deprived all important operations of their terrors. Certainly the preparations for an operation are often tedious, the first cleansing and preparation of a fresh wound circumstantial and lengthy, but with the first dressing an immense amount of work has been obviated, which formerly extended over the following weeks

and months. The later course, the fate of the wounds of the patient, is as a rule determined and assured by this first dressing; we can often heal an amputation wound with one or two dressings.

Our views on the nature of contagia in the surgical department of hospitals, on their origin and breeding places, have entirely changed. There is no hospital that is so bad that it can excuse failure in the treatment of wounded and operated patients. If we had no other task in the treatment of our sick but to guard against the purely relative interruptions of the normal process of wound healing, we could be content with the old close and over-filled houses, and the splendid piles we now erect would be superfluous. We well know plague-stricken hospitals, where, from wound to wound, from sponge to sponge, from compress to compress, fungous formations in countless generations have grown to the highest degree of adaptation to the fluids and wound secretions of the human body. But it lies in our power to disinfect them. By no ventilation in the world can septicæmia, pyæmia, and erysipelas be blown out of the hospitals; not even by the most assiduous care in ordinary life can they be avoided, but only by the conscientiousness and prudence of the surgeon. If ever the excellent Simpson was right in his assertion that far more operated patients die in hospitals than in private practice, modern surgery may rather declare the contrary. If there is any danger of a mistake in the treatment of a wound, it is more likely to happen in a private house where it is more difficult to satisfy the accurate demands of modern surgery than in the hospitals.

Gentlemen, weighty circumstances and daily occurences have always made surgeons as well as laymen recognize the importance of those problems which are solved by the discovery of the mycotic nature of wound diseases. The contrast between open and subcutaneous wounds must have been known as long as men have worked hard and have fought each other with weapons. And not only was this known, that simple fractures are not dangerous and heal without suppuration, but also the experience had been gained that there will be no suppuration, no matter if the individual concerned is young or old, robust or decrepid, scrofulous or syphilitic—whether he has this or that constitutional condition, this or that individual disposition. And this also has long been known, that the danger in a compound fracture is very great; if only on the lower thigh the skin, very poorly supplied by nerves and vessels and scarcely for an inch in area be sharply perforated by one of the fractured ends. But the consequences of these facts were not seen. They were too much opposed to the prevailing doctrine which regarded not only the suppuration, but also the serious disturbances resulting from it, as only *quantitative* increase of the process of reaction following on the injury. The whole of antiphlogistic therapeutics was based on this supposition. And yet the history of fractures might long ago have afforded the most conclusive proofs that it could not be the intensity and extent of the injuries of the tissue which determine the severity of the inflammation and the suppuration, that these latter had in reality nothing to do with the traumatic reaction, but must be some other thing essentially different. For even a good number of simple fractures are by no means such simple things as was formerly imagined, since they often do not occur without serious crushing of the marrow, tearing off of the periosteum, laceration of muscles, fissures, and splinters; certain forms of fracture, especially those of the cervix femoris, and of the spine, are good examples of destruction and crushing of the tissues. And yet even in these cases, suppuration of the greatest intensity does not occur. To attain the conviction that mechanical obnoxious influences, however severe, cannot of themselves produce suppuration, no myologic examination would have been required. The importance of this fact was only revealed to me in its full bearings in examining the wounded on the

Bohemian battle-fields. Among the great number of gunshot fractures which I had to treat there, there was a not inconsiderable number of cases where cure resulted after the usual course, as after a subcutaneous fracture without suppuration and without discharge of splinters; and I considered it obvious that here only that explanation was admissible which Stromeyer had given to these cases —namely, that the ball, passing at a tangent, had broken the bone flat, as in the simplest subcutaneous fracture. But of those so successfully cured, two of those who had been injured in the upper part of the thigh died from the results of other wounds inflicted at the same time; and when I examined these supposed bone-fractures, I found to my surprise that just here the most extensive destruction had taken place. In the one case twelve splinters, in the other about twenty, of a size varying from several inches to that of a pea, were caked together by fresh callus without any suppuration or necrosis, and in another similar case there lay firmly inclosed in the callus a little piece of cloth from the uniform.

Then, in the year 1870 Professor Klebs discovered that even in the internal organs, which are considered so vulnerable—as brain, lungs, and spleen,—no inflammatory reaction occurs, if the path of the bullet is so long and narrow that it prevents the admission of air; and that even weeks afterwards, macroscopically, no changes can be traced in the tissues shot through, while microscopically the process of healing can only be recognized by a minimum of cellular activity.

And now antiseptic experiments which exclusively belong to Mr. Joseph Lister, to whom I therefore here offer a small part of the gratitude that German surgery owes him, show us that, after an amputation, on the fourth or sixth day the ends hang down in just as limp and unchanged a manner as immediately after the operation, just as if it had been performed on a dead body and not on a living being; that after a great plastic operation, in spite of countless sutures, no trace of œdema or reddening, and no pain, occur. India-rubber tubes passed through dropsical joints may remain lying there for weeks without causing suppuration, and they may, slipped unnoticed into the cavity, heal so firmly into the armpit that a year after they may be mistaken for well-defined cancer tubercles, and be extirpated. Into the open peritoneal cavity a piece of gauze larger than a table napkin, and pressed together like a handkerchief, can be put, and remain for several days, without fever or clinical appearances of fever resulting. And I should like to mention one more experiment, which, it is true, will not always succeed. Suppose we have a case of limited fistulous caries of the foot before us; five or six times we introduce the glowing Paquelin cautery iron to the depth of an inch and a half, and carbonize the dissected tissues. Then we cut the skin in a circle round the former fistula at a breadth of two lines, and let the blood, mixed with a disinfecting fluid, flow into the deep spherical cavity, fill it out, and coagulate. In a fortnight or three weeks the upper layer of the coagulum scales off and the burn is cicatrized without any reaction, suppuration, or falling off of the scab. So that in man also Professor Hueter's five experiments on animals are confirmed.

After such facts there can be no further doubt that we have hitherto studied traumatic reaction of open wounds on tissues infected at the same time that the four cardinal symptoms of inflammation—robor, calor, tumor, dolor, even in the severest open wounds—are not developed when disturbing agencies have no access, or do not find soil fitted for their development.

Seriously, we have now to consider the question whether mechanical irritations, such as the surgeon's knife and saw cause, and the injuries of the social life, are at all capable of alone bringing about those processes which have at all times been designated as inflammatory.

I do not consider this latter question as yet fit for decision, although the ex-

treme radicals already advance weighty reasons to refuse even to thermic and chemical irritants the power of causing inflammation; so long as in the erythema of burns and the dermatitis from iodine painting associated with abundant exudation of leucocytes the influence of organic germs is not proved, and so long as the same proof is wanting for carbolic erythema, I believe that we ought at any rate to wait for the results of further investigations.

Gentlemen, I have touched on traumatic reaction of the tissues to show how distinctly modern times have influenced even the oldest dogmas of our science; how we are obliged to abandon almost everything that our schools have taught us in order to gain for our scientific opinion a newer, firmer foundation.

I am coming to an end. I have already occupied your attention for a longer time than may appear fitting to some of you. I have spoken of the advances in surgery. There is one I have not yet mentioned. It has never hitherto been so deeply and universally realized that the interests of our practice must coincide entirely with those of our patients; never before was the feeling of responsibility for our actions so great as at the present day. The breach which divides us from the older surgeons is immense here. We do not refuse to let our capability be entirely measured by our successes. For even if the surgeon of former times willingly submitted to this measure, when it was only a subcutaneous fracture without essential displacement or the straightening of a club-foot that was to be dealt with, yet, as soon as it was a matter of preserving life after an operation, or even of preventing serious disturbances, he might decidedly reject this standard. No surgeon, however learned and skilful he might be, would have been willing then to promise or guarantee, or in case of ill-success to reproach himself; he is almost certain in this important point to have, like every other surgeon, the sad privilege of not being responsible for uncontrollable circumstances.

To-day we may say with the deepest conviction that the surgeon is responsible for every disturbance that occurs in a wound; that it is his fault if even the slighest reaction or redness is developed in it, or if an amputation is not healed by first intention. He must reproach himself severely if after an operation bagging of pus occurs, and especially if death occurs from pyæmia. He who cannot attain to this degree of perfection may be converted from his former method of treatment to antiseptic methods like one who, having hitherto always prescribed senega for catarrh, now uses ipecacuanha. Of the storm that has swept over the fields of surgery during the past ten years he has not experienced a breath.

Justice requires us to recognize that the higher moral aspect of this question has come to us from England. It was the English ovariotomists, with Mr. Spencer Wells at their head, who first showed us how to approach an operation at that time looked on as fatal, how to take on one's self the whole responsibility of success or failure, and to keep no other goal in view than this one, to save the patient, and, by a constantly improving and perfecting of our art and carefully keeping everything injurious at a distance, gradually to become more and more master of one's actions. At that time we began to recognize that at the operating-table there are other matters of importance than to shine by quickness, elegance, and security in the use of the knife.

Yes, gentlemen, our labours have become more blessed and more joyful. We know that a great responsibility rests upon us, but we also know that with honest will, knowledge of our subject, and use of all our powers, we are able to satisfy its demands. And because we believe that this feeling of responsibility is living and constantly increasing in the younger generation, we trust that surgery will overcome the dangers that doubtless lie in the rapid changes it has experienced, and is being led towards a further development.—*Lancet*, Aug. 13, 1881.

MONTHLY ABSTRACT.

Anatomy and Physiology.

An Experimental Inquiry into Human Milk, and the Effects of Drugs during Lactation on either Nurse or Nursling.

Mr. THOMAS M. DOLAN and Mr. W. H. WOOD commence, at p. 85 of the *Practitioner*, for 1881, a series of interesting papers on this subject, undertaken by Mr. Dolan in order to answer the frequent question, "Will my medicine have any effect upon the child?" That medicines and foods do affect the milk, and consequently the child, is certain. Frequently, a wet-nurse, on her first arrival, causes the child to thrive; but soon all becomes changed, because, from a laborious out-door life, with few luxuries, the woman is surrounded with good living, and has very little exercise, and consequently her milk ceases to nourish the child. Cause her to resume, as near as possible, her former mode of life and food, then her nursling quickly recovers. A clear description of the best method to analyze human milk is then given, which is, however, too long for insertion, and cannot be condensed. The practical outcome of these elaborate papers is summed up at page 337. 1. All therapeutical agents intended to act on the mammary gland must first enter the blood. 2. All drugs derived from the natural orders Liliaceæ, Cruciferæ, Solanaceæ, Umbelliferæ, etc., enter the blood and impregnate the milk, hence caution is needed in giving such drugs to nursing women. 3. The only approach to a true galactagogue is jaborandi. 4. Belladonna is an antigalactagogue. 5. In inaction of the mammæ, the milk may be increased and influenced by medicines. 6. The milk of the mother may be increased in heat-forming elements by the administration of fats. 7. The salts of milk may be improved by the administration of medicines. 8. Various physiological actions—purgative, alterative, diuretic, etc., may be produced in the child by administering drugs to the mother. 9. If we are to expect any improvement in milk-secreting power, both as to quantity and to quality, we must look to diet for the attainment of that object.—*London Med. Record*, July 15, 1881.

Materia Medica and Therapeutics.

Pilocarpin.

Dr. WILLIAM SQUIRE read a paper at the London Congress on the physiological action and therapeutic use of pilocarpin, which is the alkaloid obtained from the leaves of jaborandi, on which the efficacy of that drug, long known in Brazil, in producing sweating and salivation, depends. Another alkaloid, of different, and even antagonistic properties, named jaborin by Harnack and Meyer, coexists with it, and renders the infusion or tincture of jaborandi less certain, and perhaps, less safe than that of the pure alkaloid. It is possible that pilocarpin itself has not always been obtained quite free from admixture with its associated but antagonistic jaborin. Muriate of pilocarpin, in simple solution, is the best form to use —1 grain to 15 minims of water for hypodermic injection—1 grain to 4 ozs. of water for internal use are convenient proportions; ¼d grain is the largest, $\frac{1}{12}$th of

a grain the smallest dose needed. Mr. S.'s plan is to give a full dose at once; others give small doses every hour with some warm drink or alcoholic stimulant, till perspiration and salivation are freely established. A drachm of the tincture, made with 30 grains of leaf, is equivalent to ½d of a grain of pilocarpin; ¼th of a grain of the muriate, injected hypodermically, will in a few minutes produce suffusion of the face, quickened pulse, some throbbing in the neck, and a general feeling of warmth, followed by free perspiration; this is soon streaming profusely from all parts of the surface, and continues long after the skin has become pale, or even cool; the pulse subsides, while the force of the heart's impulse is rather increased; there is a tendency to sleep, and generally a fall of temperature; the perspiration goes on for three or four hours; there is an increased flow of saliva, and some increase of pharyngeal, and sometimes of bronchial mucus, that may give rise to trouble during sleep, and require attention; such a quantity of saliva may be swallowed as to excite vomiting. No headache, sickness, or depression has been noticed by me as a direct result of this medicine. All the secretions of the body, except the intestinal, are increased by it; the quantity of urine, hardly lessened during perspiration, is increased afterwards. Dr. S. has not met with dysuria. Swelling and tenderness of the submaxillary salivary glands has remained for a day or two after profuse ptyalism. The action of the drug is on the peripheral secreting apparatus, and not on the nerve centres, except so far as the first action on the vaso-motors may dilate the vessels, and allow the agent freer access to the glands.

Atropia is directly antagonistic to it in this respect. Aconite dilates the vessels, but weakens the heart, while pilocarpin, having no such effect, may even allow of small hemorrhages occurring. It is not anæsthetic. The perspiration induced by it does not relieve dysmenorrhœa, sciatica, or colic. Pilocarpin does not moderate specific fevers; but given near the time for the separation of the false membranes in diphtheria, it aids the fall of temperature, and favours sleep. Where there is already a tendency to collapse, of course it can do no good. It is useful in the febrile relapse of scarlatinal nephritis. Dr. S. has met with no confirmation of the observation that small doses (¹⁄₁₀th of a grain) will check perspiration; a similarly homœopathic view, that particular diseases must have particular remedies, has led to the statement that pilocarpin is unsuited to renal disease. The use of it has been chiefly in the different kinds of Bright's disease; it may be unsuited to that particular form where dilated vessels and diminished blood-pressure are associated with a large quantity of albumen; yet, in these very cases, it is serviceable to the intercurrent exacerbations and conditions of accidental congestion, not infrequent in their course, and it is preferable to the hot pack, or vapour bath. In the early stages of interstitial nephritis of gouty origin it is of great benefit; in the chronic course which these cases generally follow, it is often useful; it may be resorted to in some of the extreme effects of renal dropsy, and the relief obtained is not accompanied by great depression. In the chronic results of parenchymatous nephritis, as after scarlet fever, it has been found useful; and that it need not be withheld in some cases of scarlet fever itself is proved by the remarkable results obtained from it by Guttman in the treatment of the allied disease diphtheria.

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Physiological and Therapeutic Action of some new Active Principles (Pelletierine, Valdivine, and Cedrine).

Dr. Dujardin Beaumetz was the author of a paper with the above title, read at the London Congress, of which the following is an abstract:—

Pelletierine is the alkaloid obtained from pomegranate-bark. It was discovered by Tanret, on the 26th of August, 1878. Its name commemorates the services of the French chemist, Pelletier, the discoverer of a large number of alkaloids

and, in particular, of quinine. Pomegranate root contains a series of alkaloids: Pelletierine ($C_{16}H_{15}NO_2$), Isopelletierine ($C_{16}H_{15}NO_2$), Pseudopelletierine ($C_{18}H_{15}NO_2$), and Methyl-pelletierine ($C_{18}H_{17}NO_2$). The author has investigated the action of all the above alkaloids, more especially of the sulphates of pelletierine and isopelletierine.

To the lower animals these alkaloids are speedily fatal. Rabbits are killed by a dose of 0.15 to 0.20 centigramme; frogs, by the hypodermic injection of one or two drops of a solution of one in ten. These effects are due to a paralyzing action upon the motor nerves, on muscular contractility, and on sensibility. The alkaloids are "curarizing poisons;" there is no difference between a curarized frog and one poisoned by the salts of pelletierine.

In the human subject, the sulphate of pelletierine, or of isopelletierine, administered hypodermically in a dose of from 30 to 40 centigrammes, gives rise to motor paresis with vertigo; the latter being due to well-marked congestion of the fundus oculi, and of the whole encephalon.

Pelletierine has been chiefly used as a remedy for tape-worm. The author and M. Bérenger-Féraud have shown that, of the four alkaloids, pelletierine and isopelletierine alone are endowed with tænicide properties. The author prescribes the sulphate of pelletierine in combination with tannin. A mixture of 30 centigrammes of the sulphate, in a solution containing 90 centigrammes of tannin, is given on an empty stomach; this dose is followed by one of 30 grammes of compound tincture of jalap. This treatment is followed by the expulsion of the tapeworm, with its head, in a majority of cases (9 out of 10). The above dose is only suited for adults. Children about thirteen years of age should not take more than 0.15 centigramme of the sulphate of pelletierine, and infants not more than 0.10 centigramme.

The author has prescribed pelletierine with success in Menière's vertigo; he recommends it in all those cases in which curare is indicated (tetanus, rabies) and in those ocular affections in which it may be necessary to excite rather active congestion of the fundus.

Valdivine and cedrine have been extracted by Tanret from cedron seeds. Of these seeds there are two distinct kinds, which have hitherto been confounded with each other. One of them is derived from the simaba cedron (Simarubaceæ), the other from the valdivia, also a simarubaceous plant, but which belongs, according to Planchon, to the genus picrolemma (picrolemma valdivia). Both of these species grow in Columbia.

Valdivine ($C_{16}H_{24}O_{20},5HO$) crystallizes in hexagonal prisms. It is not, strictly speaking, an alkaloid, but a glucoside, not entering into combination with acids. It requires 30 parts of boiling water for its solution. Its bitterness is intense.

The author has investigated the action of this substance together with M. Restripo. It is extremely poisonous, a dose of 0.002 milligramme being sufficient to cause the death of a rabbit in ten hours. Increase of dose does not much increase its toxic activity, which appears to be greater in proportion to the place of the animal in the scale of organization. Thus, frogs are not susceptible of the poisonous effects of valdivine.

In the human subject, a dose of 4 milligrammes, given subcutaneously, excites vomiting in from one to two hours.

The administration of valdivine in cases of intermittent fever, snake-bite, and rabies, yielded negative results in all save the last-named affection. Experiments made at the veterinary school of Alfort showed that a dose of 4 milligrammes of valdivine administered to rabid dogs abolished the paroxysms, without, however, preventing the fatal issue of the disease.

Cedrine has been extracted from cedron seeds. It was first described by Lewy,

who seems, however, to have confounded it with valdivine. Cedrine has not been crystallized. It is much less poisonous than valdivine, 10 milligrammes being required to kill a rabbit in forty-eight hours.

Cedrine does not cause vomiting. Subcutaneously injected, in doses of from 3 to 5 milligrammes per diem, it exerts a real influence on intermittent fever. But its activity is inferior to that of sulphate of quinine.

Against rabies and snake-bite cedrine is absolutely devoid of efficacy.

Medicine.

Connection of Chorea with Rheumatism.

Prof. STEFFEN, of Stettin, made some interesting remarks at the London Congress, on this subject, of which the following is an abstract:—

A definite interdependence between chorea and rheumatism is not as yet proved. That both of these morbid states are often joined with endocarditis cannot be used as evidence of such a connection.

The relation between chorea and endocarditis cannot be fixed anatomically or pathologically. Probably the chorea is always the primary morbid phenomenon.

The chief symptoms of acute endocarditis are active fever, dilatation of the heart, with enlargement of the area of dulness, a systolic blowing murmur, and an accentuation of the second sound in the area of the pulmonary artery. The dilatation precedes the murmur, if the endocarditis has originally or exclusively attacked the heart walls. In primary inflammation of the valves the opposite takes place.

When an endocarditis which exclusively involves the heart-walls recedes, the dilatation of the heart disappears first, and then, gradually, the blowing murmur. If the valves are also attacked, the systolic murmur remains after the dilatation has disappeared. Dilatation and hypertrophy may afterwards develop afresh as a secondary process.

Acute dilatation of the heart is observed without endocarditis in grave and acute obstruction of the pulmonary circulation, and in septic processes.

Cardiac murmurs occur in chorea without endocarditis. These depend on impaired function of the heart, not only through nervous influence, but also through the obstruction to the circulation of the blood, which occurs as a result of the spasmodic movements of the body.

The Modifications of Syphilis in the Tuberculous, Gouty, and other Constitutions.

At the London Congress M. VERNEUIL, of Paris, read a valuable paper on this subject, of which the following is an abstract:—

Syphilis may coexist with any other constitutional taint. There is often simple coexistence of the two conditions, but not unfrequently one has an important influence on the other.

Though referred to by Hunter, Ricord, Revillet, and others, there is, at present, no complete work on syphilitic hybrid disease.

The author's observations have been principally directed to hybrids between syphilis and scrofula and syphilis and cancer.

Scrofula usually precedes syphilis, and exerts an influence upon it; but, occasionally, syphilis in a youth or young adult, brings out struma which has been latent from infancy. Scrofula attracts syphilis to the organs it most commonly itself affects, skin, glands, periosteum, etc., and is apt to set up suppuration in

these, a result not common in simple syphilis. Scrofula modifies secondary and tertiary syphilitic ulcerations, so as to make the diagnosis often difficult. It does not aggravate syphilis, but perhaps renders its local manifestations more permanent, while, as a rule, it removes the element of pain.

Tuberculosis, on the other hand, if it does not actually favour the occurrence of severe and intractable syphilitic lesions, certainly makes some tertiary manifestations persist indefinitely. Stricture of the rectum, for example, is commonly complicated with pulmonary tuberculosis.

In treating such conditions, it is well to combine antiscrofulous with antisyphilitic remedies, but, on the whole, the indications are similar in the two diseases.

When cancer is combined with syphilis, it is the former which is modified by the latter. The cases are rare. There are hardly any which prove the existence of the opposite condition. The author has seen cancer attack a testicle in which a gumma had existed which had been cured two years previously, no doubt a *locus minoris resistentiæ*.

The diagnosis of such hybrid cases is difficult. Enlargement of the lymphatic glands favours the idea of the existence of a new growth. The almost complete indolence which is so frequently noticed, and the comparative benignity of these cases, are due to the syphilis; but the constant advance, the infection of the system, and the fatal termination depend upon the new growth, the latter always asserting itself at last. In doubtful cases, antisyphilitic remedies should always be tried. They sometimes produce remarkable improvement, and raise the hope of a cure. This temporary arrest of the growth may well surprise when it is remembered how useless, if it be not actually harmful, is the administration of mercury and iodide of potassium in ordinary cases of epithelioma or cancer.

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On the Röle of Syphilis as a Cause of Locomotor Ataxy.

Prof. W. Erb, of Leipzig, in a paper read at the recent International Medical Congress, said: "My own recently published statistics on 100 new cases of typical tabes in male adults (*Medicin. Centralblatt*, 1881, Nos. 11 and 12), showed:—

	Per cent.
Cases *without* previous infection	12
Cases *with* previous infection	88
(Amongst them, *with secondary syphilis*	59
And *with chancre, without secondary syphilis* . . .	29)

(Up to to-day—June the 1st, 1881—I have observed 13 further cases; amongst them there is *but one without* previous infection; of the remaining 12 eight have had secondary syphilis, four only a chancre; the proportion in these cases is, therefore, still somewhat more unfavourable.)

With regard to the *time of appearance of the first symptoms of tabes after infection*, my cases show, that by far the most occur from the 5th–15th year after infection; a considerable fraction, however, occurs 3–5 years after infection.

In order to control these statistics, I have made a *counter proof* extending over all male adults over twenty-five years old, of my clientèle, who do *not* suffer from tabes, and not directly from syphilis. I have carefully examined up to to-day nearly 500 persons with regard to this question, and I find præter propter:

77 per cent. were *never infected.*

12 per cent. who had formerly *secondary syphilis;* and

11 per cent. who had only a *chancre.*

The simple confrontation of these facts is forcible enough, and convincing for every one who does not shut his eyes purposely. The only possible logical conclusion from these facts is *that there must be a certain etiological connection between syphilis and tabes.*

The remark which has been made repeatedly before this, viz., that it is just

the milder and apparently harmless syphilitic infections, which are followed later on by severe syphilis of the nerves, intrudes itself also with regard to tabes, according to my observations.

The author's experiences push him more and more towards the unitarian view of syphilis. At any rate, the *absence* of most, or even of *all*, the so-called secondary manifestations of syphilis, is by no means a proof for the *non-syphilitic* nature of the previous chancre.

Only after the decision of the question in the unitarian sense, would we be entitled to state *that tabes in* 90 per cent. *of all cases is occasioned by syphilis as one of the etiological factors.*

The Analytical Study of Auscultation and Percussion, with reference to the Distinctive Characters of Pulmonary Signs.

In the Medical Section of the London Congress, Dr. AUSTIN FLINT, of New York, read a paper, the object of which was to indicate the pulmonary signs which are determinable by the analytical method of study, and the characters by which they may be readily distinguished. The auscultatory signs referable to respiration, the loud voice, and the whispered voice, were considered; and, afterward, the signs produced by percussion. The characters of the normal respiratory or vesicular murmur were brought into comparison with those of the sign known as the bronchial or tubular respiration. Under the head of bronchial respiration, a new term—broncho-vesicular respiration—was proposed to mark the different grades of solidification below the amount requisite for bronchial respiration. Cavernous respiration was shown to have characters clearly distinguishing it from bronchial respiration. Modifications of cavernous respiration, determinable by means of analysis and comparison, were distinguished as broncho-cavernous and vesiculo-cavernous respiration. A prolonged expiratory sound was stated in the paper to denote either solidification of the lung, or absence of solidification, by characters relating to pitch and quality. The existence or the absence of solidification could thus be ascertained by characters pertaining exclusively to expiration, when an inspiratory sound was wanting. The foregoing respiratory signs, referable to the loud voice, were next taken up. The distinction between simply increased vocal resonance and bronchophony was the object of analytical study. Ægophony was shown to be a modification of bronchophony, differing from the latter in an apparent distance of the resonance and an interrupted or tremulous character. Pectoriloquy, the transmission of speech or articulated words, denoted either a cavity or solidification of lung. The sounds caused by the whispered voice were considered of sufficient practical importance to form a distinct group of physical signs. The abnormal modifications of the normal bronchial whisper were as follows: (1) increased bronchial whisper; (2) whispering bronchophony; (3) cavernous whisper; (4) whispering pectoriloquy. Whispering pectoriloquy might signify either solidification of lung or a cavity. The characters associated with the pectoriloquy enabled the auscultator to decide which one of these two anatomical conditions, in individual cases, was represented by the sign. The paper concluded with the results of the analytical study of the physical signs obtained by percussion. The number of morbid signs furnished by percussion need not exceed six, namely: (1) absence of resonance, or flatness; (2) diminished resonance, or dulness; (3) increased or vesiculo-tympanitic resonance; (4) tympanitic resonance; (5) amphoric resonance; and (6) cracked-metal resonance. These were discussed in succession.

Dr. DOUGLAS POWELL (London) said it was a pity Dr. Flint did not lay down general laws, rather than refer to particular cases. Many of the sounds of prolonged expiration heard in bronchitis and emphysema were really *râles* produced

by an interruption of the current of air passing down the tubes; and often there was no such sound if it was not interrupted. With regard to bronchial and cavernous breathing, all had seen the fixed character of the chest in pneumonia, and in lungs which had caverns in them; it was impossible to explain the sounds heard over such lungs by the to-and-fro passage of air; but these sounds might occur by conduction from the glottis.

Dr. D'Espine (Geneva) thought that Dr. Flint's paper pointed to the uncertainty of always knowing whether we are listening to a bronchial or a vesicular sound. He had been taught in Germany that, when the sound came from the lung, the consonant v could be used with the sound produced; but, if from the bronchial tubes, the aspirate h could be produced with the sound. The binaural stethoscope was a great aid to diagnosis, as, by its means, the superficial sounds of the chest were best detected; but to the ear alone, or an ordinary single tube, the deeper sounds were given off.

Sir William Gull (President) asked if it were possible to diagnose a cavity in the lung by any means whatever; or to distinguish always the sounds over a cavern from those over some bronchial tubes.

Dr. Mahomed (London) said that students had great difficulty in distinguishing chest-sounds. What was wanted was a correct definition of the terms in general use; and, if the Congress were to appoint a committee to determine the meaning of these terms, it would be a good step in the right direction. For instance, "rhonchus" and *râles* were in many text-books used synonymously; also the terms "bronchial and cavernous respiration." Again, "pectoriloquy" was limited by some to the transmission of whispered sounds, as compared to that caused by the resonance of the larynx. Numerous other examples showed that the confusion of terms was extreme.

Dr. Theodore Williams (London) said that, before arriving at the conclusions which Dr. Mahomed desired, it would be necessary to unite in using and speaking of one code of instruments. All the first works on auscultation were written when one instrument, the simple tube, was in existence; but now there was in very general use a binaural stethoscope, manufactured of totally different material, which conveyed to the ears sounds totally different from those of the single one; and, further, the description of sounds detected by these instruments did not correspond.

Dr. Flint, in reply, said that, as regards bronchial and cavernous respiration, Skoda said they were the same in character; but he (Dr. Flint) thought the one was distinct from the other. Bronchial respiration was high-pitched, tubular; and the expiratory sound was also high-pitched. Cavernous respiration was low in pitch, non-tubular; and the expiratory sound was still lower in pitch, and also non-tubular. He agreed with Dr. Mahomed's suggestion. Bronchophony and pectoriloquy were often confounded; and a committee would do good, if they settled the value of the terms. As regarded the stethoscope, the binaural had a great advantage over the single one; as, although the area of sounds was limited with the former, still it distinguished chest-sounds to a remarkable degree. Dr. Flint had used it for a quarter of a century, and advised its use. Although the instrument did give off a sound of its own, still the mind soon became accustomed to the sound, and finally disregarded it.

Sir W. Gull proposed, "That Professor Ewald (Berlin), Professor D'Espine (Geneva), Dr. Douglas Powell (London), Professor Austin Flint (New York), and Dr. Mahomed (London), do constitute a committee to consider, and, if possible, to fix on, some definite terms to be used in auscultation of the chest, so as to form a uniform nomenclature; and to report the result of their labours to the next Congress, wherever that may be held." This was carried unanimously.—*British Med. Journal*, Aug. 27, 1881.

Surgery.

The Healing of Wounds.

The treatment of wounds always occupies so large a share of the attention of every surgeon, that it was only meet that the several important questions in connection with it should have been freely raised at the recent Congress. The Surgical Section devoted a whole morning to the consideration of the means best calculated to secure primary union of wounds, and although there were some differences of opinion, in the main the old classical principles were reasserted. As in so many other cases, it is well to approach this subject through the study of the simplest cases. In the treatment of a simple cut of the finger, or after the operation for hare-lip, the essential principles to observe are to adjust as perfectly as possible the clean-cut surfaces, hold them in exact apposition, and keep the part at rest and free from all disturbance until union has occurred. A simple observance of these particulars is all that is necessary to obtain union by first intention, and every one of these is essential. If the surfaces are not placed in apposition, if they are not retained in contact, or are not at rest, union by first intention does not occur. Now in all this we have not had occasion to say anything about germs, or sprays, or antiseptics. We suppose no one regards the one as dangerous or the other essential in treating such simple incisions; and Professor LISTER has himself brought forward evidence to show that healthy blood-serum and blood-clot are not favourable nidus for the development of bacteria. We may advance further to large incised wounds, such as of the abdominal wall in ovariotomy, where observance of the same principles is sufficient to secure primary union. But if we go beyond this we reach cases of larger or irregular wounds, such as in an amputation, where the conditions are seriously modified. If we attempt to carry out the treatment on the same lines, we are at once confronted with the circumstance that the *exact apposition* of the wound surfaces is impossible; the edges of flaps may be brought together and carefully united, but no suturing, be it ever so skilful, is able to place every portion of wound surface in close contact with some other portion of wound surface and retain it so. One of the necessary conditions for primary union being wanting, failure results. As a consequence of this want of exact apposition, fluid accumulates in the spaces and interstices, and may so distend them as to irritate the nerves of the part and thus lead to inflammation, or may decompose and then excite inflammation by its directly irritating properties, or, being absorbed, may lead to general blood-poisoning. In either case the wound fails to unite throughout by first intention. The difficulty thus raised has presented itself in various ways to many minds, and hence has arisen the divergence in practice. Some surgeons, such as Messrs. Savory and Gamgee, make it their aim to carry out the three cardinal principles of wound treatment as far as possible in these complicated cases, and then by simple means prevent the ills to which they are exposed. They urge that such wounds should be treated in a great degree like simple wounds; that the parts—not the edges alone—should be adjusted with the greatest care, and retained in that position, and perfect rest secured, while the fluid exuded into the unavoidable spaces should be drained off and received into some kind of dressing in which its decomposition is prevented. The special means used are pressure, drainage-tubes, and an absorbent antiseptic dressing. Another school of surgeons, while not professing to neglect the principles named, are yet so impressed with the evils attending the decomposition of retained fluids, and rightly tracing such decomposition to the influence of minute organisms introduced from without, that they concentrate their attention—in some cases their

sole attention—upon the prevention of the access of these "germs" to wounds. Here, at once, two new elements are introduced. First, some agent has to be used which acts destructively upon the "germs," and, as these germs are organic, they share to some extent the properties of all organized structures, and the strongest antiseptics are injurious in their action on the human body also. A very careful adjustment of means to ends is thus required, the imperfection of which has made us familiar with constitutional disturbance, amounting even to death, from the effects of the agent employed to ward off decomposition. The second element introduced arises from the fact that the effused fluids are organizable, and if preserved from contamination, even in large quantity, can organize and so cicatrize a wound. In one particular the divergence in practice has recently became greater, for while the one school of surgeons look upon primary union of a wound as the surest safeguard against septic mischief, the other school, confident in its own power of preventing septic mischief, will forego the primary union of a wound in attaining this end. The difference between the two parties may, indeed, be roughly expressed thus: that while the one, confronted by complex conditions, proceeds as far as possible along the lines observed in the simpler, the other attacks them by entirely novel, methods. We say "roughly" advisedly, because, as was well pointed out in one of the recent debates, where primary union is achieved, sepsis is prevented by that very condition. Which of these two lines of practice will ultimately prove the more successful it would be foolish and useless to attempt to predict. At present the general statistics of the one bear comparison with those of the other, and it may be that each will have to accept a part of the other, or seek its aid in certain classes of cases.

We have, of course, been referring to so-called "Listerism," but we have purposely avoided using that name, and one reason for so doing is that it is unjust to so great a man to use his name in connection merely with a special means of dressing wounds. We consider Mr. Lister's work as far too great to be narrowed down to be a mere matter of spray and gauze. It is not a mode of dressing wounds that he has introduced, but a surgical revolution that he has accomplished. There is not a hospital in the whole civilized world that has not been affected by his teaching and labours, even if a spray, or gauze, or even carbolic acid, have never been found in its wards. He has gone far towards abolishing suppuration, and taught us all to consider it a preventable accident in the healing of an operation wound. And it is to him above all others that we owe it that septic diseases are now generally held to be avoidable, and their occurrence a serious reproach. His work, in fact, has been far greater than the perfecting the form of dressing that bears his name, and he can fairly count among his disciples many whose outward ritual differs *in toto* from his own.

It is interesting to notice the development which the antiseptic method of dressing wounds is undergoing. Carbolic acid has evidently had its day; the cause of immense good, its use has yet been attended with such inconveniences, and in some cases peril, that it has been, or shortly will be, replaced by other and safer means. The spray, which by some has been considered one of the pillars of the system, has been abandoned by many of Mr. Lister's most ardent followers, and he himself has spoken of it as likely to become unnecessary. The gauze, too, is being replaced in some hospitals by more absorbent, softer, and more elastic materials; so that Esmarch—an antiseptic surgeon whose brilliant statistics would be incredible from any less trustworthy source—dresses his cases far more after the fashion of Mr. Gamgee than that of Mr. Lister, and has shown that the transition from one to the other is neither difficult nor illogical. We cannot doubt that, amid all this development and change, surgery is advancing, and truth is being preserved; and Mr. Lister is far too wise a man to think that

finality has been attained in the so-called "Listerian" dressing. Side by side, however, with this development, another fact is prominent. Our hospitals, previously dirty, have become clean; surgeons, before too careless, are now scrupulous in their attention to cleanliness in all its details. And, as Mr. Lister himself says, it is "bits of dirt" that do the harm. Is it that his crusade has to a very large extent—would we could say entirely!—prevented these "bits of dirt" from being deposited on or in wounds, and that so previously necessary antiseptic precautions are now dispensed with in safety? Or did his earlier teachings err in considering the invisible floating particles in the air as more dangerous than they really are? Whatever may be the truth in the matter, it seems plain that the great lesson of antiseptic surgery has been widely learnt, and bids fair soon to be universally mastered. We now want to assign it its proper place in the management of wounds, as one of the elements—not the sole—to be considered by the surgeon; and it will not be surprising should the next few years witness a great simplification of its details as a juster conception of its influence and worth is attained. The danger has been that the "system" should blind some of its followers to the fact that Nature works along certain lines or according to certain principles, all of which must be understood by the successful surgeon; and surely when primary union of a wound is dispensed with in order that the "system" may be carried out, this danger has become real indeed!—*Lancet*, Sept. 3, 1881.

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Treatment of Detachment of the Retina by Injections of Pilocarpine.

Dr. Dianoux, of Nantes, stated at the London Congress that he had treated sixteen cases of detachment of the retina by methodical injection of nitrate of pilocarpine only, of which there were cured 6, improved 8, and unsuccessful 2. In his opinion treatment by methodical injections of pilocarpine should be employed from the beginning in all cases of detachment not symptomatic of hopeless disease. It is greatly superior to other modes of treatment. It is harmless, and in some cases no other plan is applicable. The dose should be strong at first, then moderate (salivation of at least two hours' duration). There should be a series of fifteen consecutive injections; and then a rest of eight days, etc.

Treatment should be kept up at least three months. When the result is favourable at first, the treatment should be continued till a decidedly stationary condition is reached.

The case should be kept under care and the injections resumed if there be the least evidence of a relapse.

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The Relation between Ophthalmoscopic Conditions and Intra-Cranial Disease.

Dr. Bouchut, of Paris, in some remarks before the London Congress, held that all the important diseases of the brain and cord, as well as the serious diathetic diseases, may be recognized by ophthalmoscopic examination, and he applies the term cerebroscopy to this use of the method.

Thus, congestion and swelling of the optic nerve indicate congestion of brain, meningitis, compression of brain, or commencing spinal disease. Œdema of disk and neighbouring retina shows œdema of meninges and obstruction to circulation in the sinuses and meningeal veins, in tubercular meningitis, in acute and chronic hydrocephalus, in cerebral hemorrhage, in certain cerebral tumours accompanied by encephalitis, etc. Complete anæmia of the nerve and retina shows arrest of cardiac and cerebral circulation. Death is thus easily diagnosticated by the ophthalmoscope. Retinal varices and thromboses indicate thrombosis of the sinuses and meningeal veins. Miliary aneurisms of the retinal arteries show miliary aneurisms of the brain.

In fevers and diseases of the nervous system retinal hemorrhages indicate either compression of the brain by a copious effusion, the hemorrhagic diathesis, cardiac obstruction to the cerebral circulation, or changes in the cerebral and retinal vessels caused by chronic albuminuria, glycosuria, syphilis, and leucæmia. Miliary tubercles of the retina and choroid show tuberculosis of the brain or meninges. Lastly, in nervous diseases, atrophy of the disk or sclerosis of the optic nerve always indicates a disseminated sclerosis of the brain or of the anterior columns of the cord.

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On the Relations between Adenoma, Sarcoma, and Carcinoma of the Mammary Gland in the Female; their Diagnosis in the Earlier Stages of Disease, and the Results of their Treatment by Operations.

Dr. SAMUEL W. GROSS, of Philadelphia, in a paper read at the London Congress, presented the results which he has obtained from an elaborate study of this subject, and offered the following propositions:—

1. That from a genetic stand-point there is a distant connection between adenoma and carcinoma, since they both originate from the glandular constituents of the mamma. In the former neoplasm, however, there is a numerical increase of the lacteal glands; in the latter, there is merely a multiplication of the epithelial cells, the descendants of which extend into the lymphatic vessels and the perivascular sheaths of the bloodvessels. From a clinical standpoint, adenoma is a benign tumour, and carcinoma is a malignant growth.

2. That sarcoma has neither a genetic nor a structural affinity with adenoma or carcinoma, but that it resembles the latter in its malignant attributes.

3. That, in view of the recurring tendency of adenoma after simple enucleation, the entire breast should be extirpated with it.

4. That surgical intervention in sarcoma and carcinoma not only retards the progress of the disease by preventing local dissemination and the development of visceral tumours, but it also not infrequently results in permanent recovery.

5. That local reproductions in sarcoma and carcinoma do not militate against a final cure, provided they are freely excised as soon as they appear.

6. That lymphatic evolvement does not forbid operations in carcinoma, since infected glands were removed in nearly one-third of the examples of permanent cure.

7. That the subjects of sarcoma and carcinoma are, almost without exception, safe from local and general reproduction if three years have elapsed since the last operation.

8. That all sarcomata and carcinomata of the mammary gland, if there are no evidences of metastatic tumours, and if thorough removal is practicable, should be dealt with as early as possible by amputating the entire breast and its integuments, and dissecting off the subjacent fascia. In carcinoma, moreover, the axilla should be opened with a view to its exploration and the removal of any glands which were not palpable prior to interference.

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Etiology of Aural Exostoses and their Removal by a New Operation.

Dr. JAMES PATTERSON CASSELLS, of Glasgow, read a paper on this subject at the London Congress, of which the following are the conclusions:—

The osseous tumours of the external meatus are of two kinds, the one, named hyperostosis, being a hyperplasm; the other, exostosis, being a new growth. These differ from each other in origin, site, shape, structure, and number.

Hyperostosis is never seen till the osseous meatus is completely ossified; exostosis appears before the complete ossification of the meatus. Exostosis is found

arising from a point near the junction of the osseous canal with its cartilaginous portion; hyperostosis is seen only in the inner, or osseous end of the external auditory canal. Hyperostosis is always conical in shape, never pedunculated; in exostosis, on the other hand, there is always a pedicle, and its shape varies. Hyperostosis is of ivory hardness; exostosis before complete ossification has taken place in the tumour, can be pierced to a varying depth. Hyperostosis is not movable on pressure; exostosis is slightly movable, even when complete ossification has taken place. Hyperostosis is often seen without any other disease of the ear, and if an ear disease exists, there is no causative relation between them; they exist altogether independently, and apart from each other. Exostosis is nearly always complicated with another affection of the ear, past or present. Hyperostosis, therefore, may exist in the meatus with normal hearing. Exostosis, on the other hand, is almost always attended by a defect in the hearing. The deafness which accompanies hyperostosis, in the absence of any other disease of the ear, is due to the size of the growth, and is mostly mechanical, or it is due to the presence of *débris* between or behind the tumours. In exostosis the defect in the hearing may also be mechanical; this defect, however, is generally due to ear disease, either past or present.

The operation for the removal of hyperostosis is only justifiable, when its mechanical presence has been ascertained to be the sole cause of the deafness, or when a coincident ear discharge exists, the escape of which may be hindered, or altogether arrested by the presence of the tumour. The commonest cause of deafness in hyperostosis is the presence of *débris* around the tumours, either cerumen, epidermic masses, or other matters, or to mechanical irritation and inflammation of the tissues that cover them. The hearing is mostly restored on the removal of the *débris* or inflammation. The operation for the removal of hyperostosis is best effected by a mechanical drill, such as dentists use; this is the safest method of removal.

For the operation of the removal of an exostosis a gouge is the best instrument, because the tumour can be removed at one operation, whereas a hyperostosis usually demands several operations, as well as separate sittings, for its complete removal, when this is possible. There may be several hyperostoses in an ear, but hardly ever more than one exostosis. Both classes of tumours may exist together in the same ear.

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Laparotomy and Cystorrhaphy in Cases of Perforating Wound of the Bladder.

Dr. E. VINCENT, of Lyons, presented at the London Congress the results of a new series of experiments performed upon rabbits, and the following conclusions which he deduced therefrom:—

I.—1. The contact of urine with the peritoneum is not such a fatal accident as is usually supposed.

2. Suture of the bladder by interrupted metallic stitches, the serous surfaces being brought into contact, and the stitches left in the abdomen, may be practised with almost a certainty of success.

3. Whatever the cause of rupture, the animal can almost invariably be saved if the vesical suture be practised immediately or within a very short time, even if grave complications be present.

4. The animal may be saved even if a considerable time be allowed to elapse after the injury

5. When the operation was delayed longer than sixteen hours, some animals died from urinary intoxication without true peritonitis; others survived owing to a spontaneous closure of the opening in the bladder.

6. The spontaneous closure is exceptional, and, therefore, laparotomy and cystorrhaphy should be at once practised.

II.—Taking into consideration these and previous experiments, and the result of a case in which a large piece of the bladder was excised accidentally during an ovariotomy, the author would formulate the following propositions as applicable to the human subject:—

7. Considering the almost invariable mortality which follows wounds of the bladder, however produced, laparotomy and crystorrhaphy should be resorted to immediately. The chance of success diminishes in proportion to the length of time that has elapsed since the accident.

8. As the employment of antiseptic means has removed the danger of operations involving the peritoneum, ought we not to prefer suprapubic lithotomy to any of the perineal methods, retaining only two operations for stone; lithority, if it be friable and small; suprapubic lithotomy, if it be very large or hard?

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Pathology and Treatment of Genu Valgum.

Mr. Bernard E. Brodhurst was the author of a paper on this subject read at the London Congress, of which the following is a full abstract:—

Genu valgum is regarded as a constitutional rather than as a local defect; and it is shown to be accompanied for the most part by a relaxed condition of the ligamentous system in general, and to be preceded by flat-foot. Especially the ligaments on the inner side of the ankle, and those in the sole of the foot, having yielded, the arch of the foot becomes flattened, and the foot itself everted; the inner malleolus projects inwards and is lowered towards the ground, and the shaft of the tibia is inclined outwards.

Thus the weight of the body is thrown unduly to the inner side of the limb, and is borne rather on the inner margin of the foot than on the entire breadth of the sole of the foot.

Secondly, the long axis of the femur becomes inclined inwards, the inner condyle protrudes, and the internal lateral ligament yields to the superincumbent weight, and the weight of the body being no longer borne in a vertical line and transmitted through the long axis of the femur and the tibia to the arch of the foot, is thrown to the inner side of the knee and of the foot. These, then, being the relative positions of the femur and the tibia, the former inclined inwards and the latter outwards, the condyles of the femur necessarily assume an oblique position from without inwards.

In the normal condition of the bone, there is usually some difference in the length of the condyles: in a rickety bone the inner may be one inch longer than the outer condyle; in the rickety bone the inner condyle is nipple-shaped, and the outer is undeveloped, or almost absent; so that the superincumbent weight could never have been borne on the foot, but it was probably borne on the knee. The length of the inner condyle varies according to the healthy development of the bone. The difference in length may be less than 1-10th of an inch, or it may be in an ordinarily healthy bone as much as half an inch.

In its commencement genu valgum is only apparent in the erect position, and it ceases in the horizontal position, just as is observed in scoliosis and in flat-foot; in an early stage of development these disappear in the horizontal position.

After some time, knock-knee becomes permanent. Even now, however, splints firmly supporting the knee, together with the horizontal position, are sufficient to remove this deformity.

As deformity increases, the biceps femoris becomes tense, and the ilio-tibial band and the external lateral ligament are rendered rigid. Then the right line of the limb being destroyed, the question arises how is it to be restored, so that

it shall pass through the long axis of the femur and of the tibia, and impinge on the arch of the foot?

Various operations have been proposed to restore the straight line of the limb, such as forcible straightening, osteotomy, and tenotomy.

Tenotomy, however, or that operation known as Guérin's, is sufficient; without either force or osteotomy, to remove the most severe forms of genu valgum.

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Perforating Ulcer of the Foot as connected with Progressive Locomotor Ataxy.

Prof. BENJ. BALL, of the Paris Faculty, and Dr. THIBIERGE, presented the following conclusions in a joint paper on this subject read before the London International Medical Congress:—

1. Perforating ulcer of the foot is, in such cases, the consequence of the spinal disease, as in the "joint disease," which has been brought before the medical public by Charcot and myself.

2. The local disease is more especially connected with certain symptoms of locomotor ataxy, such as shooting pains, absence of tendon reflex, and other trophical lesions.

3. The perforating ulcer may be cured while the symptoms of locomotor ataxy follow their progressive course.

Midwifery and Gynæcology.

Parallel between Embryotomy and the Cæsarean Section.

Dr. G. EUSTACHE, of Lille, in a paper read at the London Congress, presented the following conclusions, which he believed should be the future rules of practice:—

Considering, on the one hand (1), the recent results of ovariotomy, and of all other abdominal sections; (2), the improvement in the prognosis of all surgical inquiries under antiseptic treatment; (3), the success of Porro's operation; (4), the immensely favourable results both to mother and child after the Cæsarean section, which have been published during recent years.

And considering, on the other hand, that embryotomy, while it always sacrifices the child, exposes the mother to as grave dangers as the Cæsarean section; that it is inapplicable in many cases of deformed pelvis, *e.g.*, when the conjugate is 5 centimetres and under.

I conclude:—

1. When the child is living at the beginning of labour, and when the pelvic strait is under 78 mm.—the extreme limit for the application of the forceps—the Cæsarean operation should be performed early, that is to say, as soon as labour has really set in, and with antiseptic precautions.

2. If the child is dead, and the superior strait measure 5 centimetres, recourse should be had to embryotomy. Below 5 centimetres the Cæsarean section becomes an operation of necessity.

To sum up, the Cæsarean section should be the method of election, embryotomy that of exception.

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The Curability of Uterine Displacements.

Dr. PAUL F. MUNDÉ, of New York, finding that the text-books either entirely omit all mention of the possibility of permanently curing displacements of the uterus by any of the methods in use, or give but vague statements on the subject,

and impressed with the importance of having some positive conclusions on this matter, both for the sake of the patient and the satisfaction of the physician, in a paper presented to the London Congress has analyzed the numerous cases of displacements which have come under his care (895), and arrives at the following conclusions:—

1. Displacements of the uterus are permanently curable in the large majority of cases only when recent, or when a complete tissue metamorphosis, as occurs during pregnancy and after parturition, takes place.

2. Chronic cases (of more than a year's standing) are but rarely curable permanently, except occasionally under the last-named circumstances. Apparent cures reported by some authors and witnessed by many physicians, soon show themselves to have been but temporary.

3. Pessaries form unquestionably the most practical, rational, and (temporarily) the most efficient means of treating uterine displacements. Cures are but rarely accomplished by them.

4. Medicated, chiefly astringent, tampons, intelligently applied every day by the physician, give the best chances for permanent cure. This is particularly true of prolapsus, but holds good for all forms.

5. Electricity locally applied deserves more extended application.

6. All methods should be persevered in for months and years before success is to be expected.

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Removal of both Ovaries for the Cure of Insanity.

Dr. T. B. WILKERSON, of Youngs Cross-roads, Granville, N. C., reports in the *North Carolina Medical Journal*, June, 1881, the case of a young woman 19 years of age, in which he successfully extirpated both ovaries for insanity with strong erotic tendencies of two years' duration. At the end of three weeks she recovered from the operation, and there was a gradual improvement in the mental condition, and three months after the operation sanity was perfectly restored. Nine months after the operation she is reported as being "gay, lively, cheeks ruddy, and in the full enjoyment of a vigorous mental and physical health."

Hygiene.

Influence of Milk in Spreading Zymotic Disease.

At the London Congress Mr. ERNEST HART, the talented editor of the *British Medical Journal*, submitted an abstract, giving in a tabular form particulars of 71 recent epidemics due to infected milk, that have been recognized and made the subject of detailed observations in Great Britain, 67 of them since the Marylebone milk typhoid epidemic, traced and reported by Dr. Murchison and the author, in 1873. The particulars of the epidemics are given in the abstract under the following headings:—

1. Date of outbreak; 2. Locality; 3. Reporter; 4. Total number of cases; 5. Deaths; 6. Number of cases amongst drinkers of infected milk; 7. Percentage to total cases (col. 6 to col. 4); 8. Number of families supplied by milkmen; 9. Number of such families invaded; 10. Percentage (col. 9 to col. 8); 11. Sanitary circumstances of farm or dairy from which milk was derived; 12. Exciting cause of outbreak; 13. Circumstances implicating milk; 14. Facts showing special incidents of the disease; 15. References to sources of information.

The three diseases which have as yet been recognized as capable of being spread by milk are typhoid fever, scarlatina, and diphtheria. There is nothing

in the analogy of epidemics to limit the list permanently to these; and already there are indications of other cognate diseases being spread by the same agency. The number of epidemics of typhoid fever recorded in the abstract as due to milk is fifty, of scarlatina fourteen, and of diphtheria seven. The total number of cases traced to the drinking of infected milk occurring during the epidemics may be reckoned in round numbers as 3500 of typhoid fever, 800 of scarlatina, and 500 of diphtheria. As regards typhoid fever, the most common way in which the poison has been observed in these epidemics to reach the milk is by the soakage of the specific matter of typhoid excrements into the well-water used for washing the milk cans and for other dairy purposes, and often, it is to be feared, for the dilution of the milk itself—for which, in official reports, "washing the milk cans" has become a convenient euphemism, advisedly employed to avoid raising unpleasant questions.

In twenty-two of the fifty epidemics of typhoid fever recorded this is distinctly stated by the reporters to be the case, and in other cases it was more or less probable.

When a dairy is unwholesomely or carelessly kept, there is obviously a great variety of ways in which the poison may reach the milk. (Numerous instances of this kind are given.)

Scarlatina being almost invariably spread by contagion and by the inhalation of the bran-like dust which is thrown off from the body during the disease, we should expect in epidemics of milk-scarlatina to receive evidence of this dust having access to the milk; and in the majority of recorded epidemics it was found that persons employed about the dairy operations were in attendance at the same time on persons sick of scarlatina.

In none of the seven recognized outbreaks of *diphtheria* due to milk, has it yet been possible to trace the exciting cause of the outbreak, though as to the disease being spread by milk there could be no doubt whatever. It has indeed been suggested whether a disease of the udder of the cow, called "garget," may not so affect the secretion of milk as to give rise to diphtheria in the human subject. So far, this notion is a mere conjecture unsupported by fact.

The great majority of the cases give statistical as well as experimental support to the conclusion that the responsibility of the epidemic lay with the milk.

It is upon the largest drinkers of the milk (those, namely, who, consuming the greatest quantities, have a correspondingly greater chance of imbibing disease germs), that the incidence of the disease chiefly falls.

Thus young children (ordinarily little liable to attacks of typhoid) who are accustomed to drink milk largely in the raw state, domestic servants, who, after children, drink the most raw milk, and large milk drinkers of every rank and station, furnish by far the largest quota of cases in each epidemic.

People, too, who drink exceptionally of the implicated milk are attacked, although the milk taken at their own houses is derived from other sources.

The houses invaded during the epidemics are found to be commonly of the better class and in healthy situations. The poor, who take very little milk, and that only in tea or coffee, commonly escape the disease.

The striking fashion in which the disease "picks out" the streets supplied by the implicated dairy, and the houses in those streets receiving the milk, is noteworthy. People in adjacent houses, and who drink milk supplied by different retailers, escape; and when supplies from two sources enter the same house, the disease only attacks those drinking that from the infected source. The contemporaneous invasion of so many households at once can only be explained on the hypothesis of a common cause acting on a particular set of persons, and on no others

MEDICAL NEWS.

The Case of President Garfield.

The publication in the current number of the *American Journal of the Medical Sciences* of the official report of the autopsy upon the body of President Garfield will, we trust, while satisfying the legitimate curiosity of the profession and of the laity, at the same time effectually and permanently quiet the unfriendly criticism of the surgical treatment of the case, in which part of the daily press has so freely indulged, and from which, we regret to observe, some medical journals without full knowledge of the case have not thought proper to abstain. The discoveries of the autopsy, taken in conjunction with what is known of the clinical history, will at once make apparent to the profession the good common sense, admirable conservatism, and sound surgical judgment which have characterized the management of the case from first to last, and, although the non-medical mind may be slower to comprehend the questions at issue, it will not be long before the same conviction forces itself upon the people at large.

We know now beyond the possibility of doubt that no human skill could have averted the fatal result, but we find moreover that, even in the searching light of the careful and thorough post-mortem examination, it is difficult, if not impossible, to suggest any modification of the treatment even in minor points, which would have made it better adapted to the exigencies of the case.

Directly after the shooting, the symptoms of impending collapse, so severe as to threaten immediate dissolution, and possibly, in the absence of hemorrhage, depending largely upon spinal concussion and mental shock, received, as was proper, the greatest attention.

Later in the day, after reaction had been established, a cautious examination with a probe and with the finger disclosed the fact that the eleventh rib had been struck and broken, and that the ball had passed onward into the abdominal cavity. In accordance with one of the best-established rules of surgery, further exploration was desisted from, and two days later, at the time of the first consultation, the same praiseworthy conservatism was observed in spite of the senseless clamour for the extraction of the ball which was already being raised, and which, to their shame, be it said, was aided and encouraged by a few prominent members of the profession who had possessed neither the details nor a personal knowledge of the case. Although, at that time, the surgeons were governed by general principles, and were necessarily ignorant of the course and situation of the

ball, the wisdom of their inactivity has been made clear by the disclosures of the autopsy, and we can all recognize now that an operation for extraction would have been not only fruitless, but almost certainly fatal, while, on the other hand, it is shown that the ball itself, after effecting its lodgment, had ceased to be a factor in the production of disease. We know also, that, as it had passed through the fibres of the psoas muscle, which, running longitudinally by the side of the spine, had closed over it, persistent probing would only have been productive of increased laceration of the parts with no possibility of the discovery of the track of the ball.

It seems not improper to say in this connection that weeks ago, when the daily press was filled with fictitious accounts of the exact situation of the ball, and of certain imaginary movements which it had made, Dr. Agnew, who, doubtless, in this, as in other matters, represented accurately the sentiment of the surgical staff, assured us that he did not know the position of the ball, and knew of no safe means of determining it.

The vomiting immediately after the accident, and the temporary retention of urine were common symptoms of shock, and received all the attention they required; so, too, the pains in the feet and ankles and the scrotal hyperæsthesia—both possible results of nerve-injury or of concussion of the spine—called for no special treatment, and subsided, together with the abdominal tympany, in from one to two weeks. At no time was there any loss of muscular power. The rigors which occurred about the end of July were immediately associated, in the first instance, with the formation of a small abscess under the erector spinæ muscles, possibly caused by the irritation of fragments of the broken rib, and next, with the obstruction of the outlet from this pus-cavity by one of these fragments. A free incision in the one case, and removal of the spicula of bone in the second, caused an immediate cessation of the chills, followed by subsidence of the temperature. It seems probable that about this time the blood received its first septic impression; but this is not to be wondered at when we recall the fact that suppuration occurring in this region is rarely circumscribed, the pus, owing to the scanty amount of connective tissue and to the presence of loose masses of fat, finding its way freely through the surrounding parts, and thus affording the best possible condition for decomposition and for the absorption of poisonous matters. When, too, we remember the laceration of the cancellated structure of the first lumbar vertebra, it seems rather remarkable that these symptoms did not make their appearance earlier, and the injustice of attributing them to improper or insufficient treatment becomes evident.

The discovery of a point of induration in the right iliac fossa, and the fact that a small flexible catheter could be carried downward from the wound in that direction for several inches, led very naturally to the harmless belief that this was probably the original track of the ball; but, while the au-

topsy shows that this was not the case, it also develops the fact that the wound proper was thoroughly drained through this channel, and that the operation of August 8th, which, by an opening below the twelfth rib and directly through the quadratus lumborum muscle, effectually drained the space between that muscle and the peritoneum, and, indeed, the whole iliac and lumbar regions, also aided in carrying away the products of suppuration from the neighbourhood of the wounded vertebra.

The drainage of the long, narrow passage into the iliac fossa by a counter-opening at its lower extremity, which it has been asserted was an indication not met by the surgeons, would have necessitated an abdominal section, displacement of the viscera, and division of the posterior layer of the peritoneum, and would have resulted in absolutely no benefit, as in the recumbent position the pus would have continued to gravitate toward the opening in the lumbar region, as it did throughout the entire case.

The fact that drainage was thorough and complete, and that no portion of the unfavourable symptoms was due to neglect in this respect, is finally and fully established by the circumstance that no accumulations of pus were found at the autopsy, either along the track of the ball or in the passage caused by the burrowing of the pus. There was no time previous to the first operation at which the accumulated pus did not pass out of the original wound, but its exit was favoured by gravitation after the two operations which brought the external openings on a lower level, and enabled them not only to drain completely the iliac and lumbar regions, but also to carry away any discharge that may have come from the fractured vertebra.

Throughout the whole treatment antisepsis was employed to the fullest possible extent, although a more unfavourable case in which to obtain its peculiar benefits could hardly be imagined.

The irritable stomach, a relic of old dyspeptic troubles, the parotitis, a not uncommon accompaniment of exhausting fevers, the bronchitis and broncho-pneumonia, partly the results of recumbency and partly of the septic state of the blood, were the subsequent complications through which the President was being so skilfully, and we may say, successfully carried, when the ulceration of the splenic artery, a slowly developing result of inflammation extending from the contiguous track of the ball, terminated the case; the rigors preceding death being doubtless due to the successive hemorrhages which are indicated by the lamination of the blood-clot found in the peritoneal cavity. The edges of the ulceration in the splenic artery were adherent to the posterior surface of the peritoneum. It will be seen that no metastatic abscesses were discovered, the only accumulations of pus which were found being a cavity on the under surface of the liver which may have been a result of contusion by the broken rib at the time of the injury, and at any rate did not communicate with the wound, and another,

one-third of an inch in diameter, just beneath the capsule of the left kidney. With the exception of the moderate broncho-pneumonia the viscera were free from all acute pathological changes.

From this general consideration of the history of the case, viewed in the light thrown upon it by the details of the autopsy, we may safely conclude:—

1st. That the treatment at the time of the reception of the injury, and immediately subsequent to it, was that rendered proper by the condition of collapse which then existed.

2d. That on reaction taking place, a sufficiently thorough and careful examination was made with the finger and the probe.

3d. That when the consulting surgeons were called in, and found that this had been done, they very properly, and in accordance with well-established and universally recognized rules of surgery, refrained from repeating that examination.

4th. That even if these rules had been disregarded, and such examination had been made, it would have determined nothing of practical importance as regards the subsequent treatment.

5th. That wherever pus accumulations had taken place, they were properly opened by free incisions made at the most dependent portions.

6th. That these incisions drained not only the course of the abscess, but communicated freely with that portion of the spine which had been penetrated, and, therefore, with the track of the ball, and the completeness of the drainage is shown by the absence of pus accumulations either in the locality traversed by the ball or in the iliac or lumbar regions.

7th. That the damage done to the cancellated tissue of the lumbar vertebra was sufficient in itself to explain the septic state of the system, which in time, and independent of the ball, which proves to have become harmless, would have destroyed the life of the patient.

If it be thought that these conclusions need to be substantiated by a more detailed acquaintance with the surgical history of the case, it may be said that such a history giving minutely all the particulars of symptoms and treatment is now in process of rapid preparation, and in our opinion will serve to confirm and strengthen the above assertions.

Of the sympathy of the nation and of the civilized world with the distinguished victim, of the universal respect and affection called forth by his quiet, patient endurance of his sufferings, of the admiration excited by the persistent, almost heroic, struggle which he sustained against overwhelming odds, of his watchful and tender nursing, of the thoughtfulness which anticipated every want and of the ingenuity which devised new appliances for his comfort or his transportation, enough has been written in well-deserved commendation.

But for the men who took the crushing responsibility of his case, who spared neither time nor labour nor health in its conduct, who remained

prudent and far-seeing and self-reliant under an electric light of criticism such as has been directed upon no other physicians in the history of the world, and who have in our opinion reflected credit upon the profession and upheld the reputation of American surgery, there has been written scarcely a word of praise, but oftentimes columns of unmerited censure.

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International Medical Congress, London, 1881.

The International Medical Congress which has just adjourned at London, was the most notable medical gathering which has ever assembled in any part of the world. The Congress was honoured by being held under the patronage of Her Majesty the Queen, and of His Royal Highness the Prince of Wales. Its membership was alike international, and representative in character; and in the magnitude of the Congress, in the amount of work it accomplished, and in the attractions which it offered to those who attended it, it surpassed all its predecessors. Official representatives were sent by the governments of the United States, France, Germany, Russia, Italy, Denmark, Norway, Sweden, and by Roumania. The Prince of Wales, and the Crown Prince of Russia showed their interest in the Congress by being present at the opening meeting, and by subsequently on the same day taking luncheon with Sir James Paget, dining with Sir William Gull, and by being present at the conversazione at the South Kensington Museum in order to meet the chief notabilities of the profession who had assembled from all parts of the world to attend the Congress. The Empress of Germany graciously expressed her personal interest in the meeting by deputing Prof. Küster, Principal Surgeon of the Augusta Hospital, to attend as her special representative.

Of the scientific work done in the Sections we can only here say that the large number of papers read were, as a rule, markedly valuable contributions to our science, and the summaries of the more important will be found in our *Monthly Abstract*, and in the current number of the *American Journal of the Medical Sciences*. Some conception of the number and extent of these communications may be formed from the fact that the official volume of abstracts printed in English, French, and German, covers over 700 closely printed royal octavo pages, and contains 325 communications, extending over the entire range of medicine and surgery. The devotion of the members to the serious work for which they had assembled was most marked, and the general sessions and sectional meetings were always largely attended, and the discussions were as full and as interesting as time would permit.

Of the addresses that were made in the general sessions that of Dr. Billings, of Washington, was received with marked favour, and the sallies of wit with which it sparkled elicited cordial applause from the large audience whose attention was closely held during its entire delivery. This

able address we have laid before our readers in full, as well as that of Prof. Volkmann, who is one of the most prominent advocates of antiseptic surgery in Germany, and that of Prof. Pasteur, of Paris, which, from a scientific standpoint, is the most valuable of all, dealing, as it does, with new and original investigations, the importance of which can be scarcely over-estimated.

The social entertainments given to the Congress as a whole, or to select parties of its members, were so numerous and brilliant as to constitute a marked feature of the meeting. Indeed such a spontaneous outburst of generous hospitality as was daily met with, we believe is without parallel in the history of any scientific or popular Congress. The magnificent hospitality of the corporation of the City of London was shown in the sumptuous conversazione at the Guildhall; and the Lord Mayor of London officially entertained at dinner at the Mansion House as many members of the Congress as the Egyptian Hall would hold. Among the many notable private entertainments it is impossible not to refer to the brilliant dinner at Willis's Rooms, given by Sir William Gull, to a select number of guests invited to meet the Prince of Wales. Among the distinguished company who were not members of the profession, besides the Prince of Wales, were observed the Crown Prince of Germany, Prince Henry of Germany, Cardinal Manning, the Archbishop of York, the Bishop of London, and the President of the Royal Society. Of the members of the Congress from America there were present Drs. Billings, Solis Cohen, Austin Flint, Minis Hays, T. M. Lefferts, and D. W. Yandell.

Sir James Paget fulfilled the duties of the Presidency of the Congress with a dignity, zeal, discretion, and grace which won for him universal praise. To the unswerving energy, and quiet courtesy of the Hon. Secretary-General, Mr. William Mac Cormac, much of the superb success of the Congress is unquestionably due, and we are happy to learn that in recognition of the value of his services in promoting the Congress the Queen has signified her intention of conferring upon him the honour of knighthood.

At a meeting of the members of the Congress from the United States, held at the close of the Congress, the following resolution was passed:—

Resolved, That we highly appreciate the privilege we have enjoyed of attending this Congress, which has been, in every sense, a great success. That we offer our thanks to the officers of the Congress for the manner in which they have organized and conducted its meetings, and also to the corporations, societies, and individuals of whose unbounded hospitalities we have had such ample experience; and that we shall always preserve the most pleasant and grateful memories of the uniform courtesy and kindness which we have received, and which will strengthen the ties of friendship which exist between the United States and the mother country.

American Dermatological Association.

The fifth annual meeting of this Association was held at the Ocean House, Newport, R. I., Aug. 30th, 31st, and Sept. 1st. The proceedings opened with an able and scholarly address by the president, Dr. James Nevins Hyde, of Chicago, on *the Relations of Dermatology to Periodical Medical Literature.* A comparison of the articles published during the last few years with those of an earlier date showed a very marked improvement. Allusion was made to the importance of correct diagnosis, and some of the diseases were referred to where mistakes are frequently made. Some of the popular errors in etiology and pathology were also considered, and the injudicious use of well-known and powerful remedies, as mercury, arsenic, and iodide of potassium, strongly denounced. "He who would specially devote himself to the investigation of the diseases of the skin requires fully as much as his fellows the amplest preparation in a finished medical education and experience, and when at last ready for his work he will discover, if he has not done so before, that he needs to be a physician in the highest sense of the term. There is no organ of the body, in health or in disease, which it may not become his duty to explore and investigate." Appended to the address, the reading of which was listened to by the members of the association with the deepest interest, was a general alphabetical index, by authors and subjects, of all original communications which have appeared in the several dermatological journals.

The first paper read was one by Dr. Charles Heitzmann, of New York, on *The Minute Anatomy of the Hair*, the ground being taken that the anatomy of this structure had heretofore been incorrectly stated by authors. Dr. James C. White, of Boston, next read a paper on the *Limitations of Internal Therapy in Skin Diseases*, referring to the differences of opinion regarding the value of internal remedies among dermatologists of equal experience, and stating that in his opinion but few diseases of the skin were under the direct control of internal medication and in any uniform or marked degree. We would encourage experimental studies, empirical or philosophical, but thought that a frank confession of our present inabilities was an essential step towards real advance. This article was followed by one by Dr. Arthur Van Harlingen, of Philadelphia, describing an interesting case of *Lymphangioma cutis Multiplex.*

The next paper was one by Dr. Louis A. Duhring, of Philadelphia, on *The Small Pustular Scrofuloderm*, the disease being undescribed and rare, and characterized by small, disseminate, chronic, pustules with a hard base of a cold violaceous tint, the eruption resembling the small pustular syphiloderm.

The second day's morning session was devoted to an interesting series of articles on leprosy, and to the valuable report of the Committee on Statistics presented by the Chairman, Dr. James C. White. The first paper was on a *Case of Tubercular Leprosy*, occurring in a native of Maryland, who had never been beyond the limits of the State, and who was of healthy parentage, by Dr. I. Edmondson Atkinson, of Baltimore. Dr. Hyde, of Chicago, presented a case of acute *Tubercular Leprosy*, and read a paper by Dr. H. D. Schmidt, of New Orleans, *On the Pathology of Leprosy.* The day's proceedings were concluded by a paper by Dr. Edward Wigglesworth, of Boston, on *Buccal Ulcerations of Constitutional Origin*, the difficulties of diagnosis between lupous, tubercular, syphilitic, and cancerous disease being dwelt upon.

On the third day Dr. Heitzmann, of New York, contributed papers on *Clinical Experience in the Use of the Solution of Oxysulphuret of Calcium*, and on the use of a *Kido-Galvano-Cautery for Depilation*, his results justifying the highest claims made for the operation.

The committee on the microscopic examination of a specimen of ainhum, presented at a former meeting, reported through its chairman, Dr. Heitzmann, that the appearances presented no pathological changes to account for the result, which can only be explained by the supposition that a system of self-mutilation is practised by the negroes.

The officers elected for the ensuing year were as follows:—

President, Dr. James Nevins Hyde, of Chicago; Vice-Presidents, Drs. George H. Fox, of New York, and W. A. Hardaway, of St. Louis; Secretary, Dr. Arthur Van Harlingen, of Philadelphia; Treasurer, Dr. I. E. Atkinson, of Baltimore.

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Tribute to the Memory of the late Dr. Otis at the London Congress.—During a discussion in the Section of Military Surgery and Medicine, on the transport of sick and wounded in the field, Dr. Gori (of Amsterdam) made reference to the death of Dr. Otis, which drew forth the following remarks from the President, Surgeon-General Longmore:—

"This seems to be a very fitting occasion, representatives as we are of the science and practice of military surgery in all countries, for us to express our profound regret at Surgeon Otis having been taken away from among us before he was able to complete the greatest of all his many valuable professional works, as he had hoped to do; and it seems also to be a fitting opportunity to convey to Surgeon-General Barnes, and, through him, to all the medical officers of the United States Army, our heartfelt sympathy with them on the great loss their medical service in particular, and at the same time military surgical science in all parts of the world, has sustained in the death of their great colleague. I say these few words in the presence of an eminent friend and fellow-labourer of Dr. Otis—Dr. Billings, who occupies an important part in the Surgeon-General's Office at Washington—and I beg to propose to the meeting that Dr. Billings be asked kindly to allow himself to be made the medium of communicating this, I may truly say, international expression of feeling—for I see plainly you all share with me in sentiments I have tried to express—to the distinguished chief at the head of the Department, and to his colleagues, on his return to Washington."

Dr. Billings, in reply, said: "In behalf of the Medical Department of the United States Army, of the Surgeon-General, and of the colleagues and friends of Dr. Otis, I desire to return thanks for, and to express the highest appreciation of, the eloquent tribute which Surgeon-General Longmore has paid to the memory of Dr. Otis. I shall not attempt to add to the eulogy which Surgeon-General Longmore has pronounced upon my friend and colleague; I can only say that I find I want words to express the emotion with which I have listened to it, and that I shall convey the message with which he has charged me, to the best of my ability. You will all, I am sure, be glad to know that before his death Dr. Otis had completed so much of the Surgical History of the War, upon which he was engaged, as relates to wounds of the extremities. There remains yet to be completed the account of the complications of wounds, such as gangrene, tetanus, septicæmia, etc. Another surgeon of the Army will be assigned to complete this history, and you can hardly conceive how difficult he will find it to prepare a report which will be the continuation of, and be constantly compared with, the work of Dr. Otis."—*Med. Times and Gaz.*, Aug. 13, 1881.

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Health of New York.—The average mortality for August has been considerably smaller than that for July. During the latter month 4378 deaths occurred, while only about 3000 were reported for August. In the week ending Aug. 6, there were 839 deaths, and in that closing Aug. 13, 849. For the weeks ending

Aug. 20th and 27th, the numbers were respectively 682 and 679. This decrease in the death-rate may in part be attributed to the low average temperature which prevailed during the month, whereby the infant mortality was materially reduced, and to the indefatigable efforts of the tenement-house medical corps appointed by the Health Board. The special physicians of the corps completed their labours at the middle of the month, after a service of five weeks' duration.

The progressive abatement of typhus fever manifested throughout the month was the source of great rejoicing among all classes of society. In the 21st week of the fever's prevalence (ending Aug. 6), there were only 2 new cases; in the 22d week, 12; in the 23d week, 4 cases; in the 24th and last week (ending Aug. 27), only 1 case. 19 new cases were, therefore, reported against 124 in May, 101 in June, and 74 in July. In the last four numbers of the MEDICAL NEWS AND ABSTRACT, the reader will find a numerical history of the endemic, and will see that the entire number of cases reported during its course was 527. During the weeks ending Sept. 3d and 10th no cases were discovered, and the disease is, therefore, regarded as arrested. The last case of typhus was discharged from the Riverside Hospital ten days since. All the contagious diseases have decreased during August. 75 cases of smallpox were reported against 99 in July, and, on Sept. 7, only 12 cases of that disease remained at the Riverside Hospital. The number of scarlatina cases fell from 575 in July to 320 in August; diphtheria from 521 to 355; and rubeola from 231 to 96. The most intense heat of the season prevailed on Sept. 6, 7, 8, and 9. On the 6th inst., the thermometer reached 97½°; on the 7th inst., 100°; and on the 8th, 103°, in the shade. The total number of insolations reported for these days amounted to about 75. On Sept. 9, 26 additional cases occurred. Nearly one-third of all the cases were fatal. Such excessive heat as that just recorded has not been experienced in New York since 1871.

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New York State Health Board held a meeting at Niagara Falls on Aug. 11th, at which Dr. Harris, the secretary, read a paper proposing a plan of State vaccination for the purpose of eradicating variola. A resolution recommending the establishment of a State vaccine farm was carried. A resolution to the effect that the State Board co-operate with the National and local boards for the prevention of the introduction of variola into the United States, by emigration, was adopted. The Board has commenced to investigate the subject of adulteration of food in accordance with the provisions of a law recently enacted by the Legislature. The Board has appointed special officers to obtain and analyze samples of pharmaceutical preparations, and of the most commonly adulterated varieties of food. Among those claiming special attention are cheese, milk, butter, sugar, quinia, beer, honey, molasses, syrups, fish, baking-powders, soda-water, ice cream, teas, cocoa, canned fruits and extracts, wines and spirits.

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Regulation of Medical Practice in New York.—On August 8th Dr. A. M. Purdy, President of the New York County Medical Society, appeared in behalf of the Society in the Police Court as complainant against three unlicensed medical practitioners. The offenders were each placed under $500 bail to await examination. The censors of the society in question are determined to deal summarily with prescribing druggists and unqualified practitioners.

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Honours to the Secretary-General of the London Congress.—It is gratifying to find that the valuable services of Mr. Mac Cormac in organizing the London Congress are being recognized outside of his own country, and the profession will

learn with pleasure that, in addition to the honour of knighthood which Queen Victoria has intimated that it is her pleasure to confer on Mr. Mac Cormac, he has also received from France the Cross of the Legion of Honour.

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Transactions of the International Medical Congress.—We learn that the Transactions of the London Congress will probably fill three bulky volumes, and that the first volume, containing the Addresses, will be put to press immediately.

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Abatement of Noxious Odours.—The Committee on Hygiene of the New York County Medical Society is at present interesting itself with the abatement of noxious odours emanating from city stables, and with the prevention of smoke from kindling-wood and box factories.

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The New York City Health Board is rigidly enforcing the act pertaining to the registration of plumbers and to the submission of their plans, for new plumbing work, to the Board for approval. The result of this commendable legislation has been to materially improve the drainage and ventilation of all recently constructed houses.

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Prize Essay of the American Medical Association.—The committee of selection appointed by the chairman of the section on Practical Medicine, Materia Medica, and Physiology, at the June meeting of the American Medical Association, have selected, and announce, as the subject for the prize to be awarded in 1883, the following question:—

"What are the special modes of action or therapeutic effects upon the human system, of water, quinia, and salicylic acid, when used as antipyretics in the treatment of disease?"

The essays must be founded on original experimental and clinical observations, and must be presented to the chairman of the committee of *award* on or before the first day of January, 1883.

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Obituary Record.—At Breslau, on August 10th, of kidney disease, in the fifty-second year of his age, Dr. Otto Spiegelberg, Professor of Midwifery and Director of the Obstetric Clinic in the University of Breslau.

Dr. Spiegelberg graduated at Göttingen in 1853, and in 1858 published his first work, a *Compendium of Midwifery*. In 1861, he was appointed Professor of Obstetrics and Gynæcology in the University of Freiburg. In 1864, he accepted a similar professorship at Königsberg; and in 1865 accepted the professorship at Breslau. Since that time, he has been distinguished for literary activity. In conjunction with Dr. Credé, of Leipsic, he established the *Archiv für Gynäcologie*, of which seventeen volumes have appeared, and which contain numerous contributions by himself and his assistants. About five years ago, he published a *Text-book of Midwifery*, the second edition of which he had almost completed at the time of his death. He distinguished himself by his readiness to adopt all new improvements in the obstetric art. The first ovariotomy in Breslau was performed by him, and he operated in all in considerably more than one hundred cases. By his death, the obstetric branch of medicine has, in Germany, suffered a severe loss.—*British Med Journal*, Aug. 20, 1881.

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To Readers and Correspondents.—The Editor will be happy to receive early intelligence of local events of general medical interest, or which it is desirable to bring to the notice of the profession. Local papers containing reports or news items should be marked.

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